# BULLETIN
OF THE
# American Schools of Oriental Research

JERUSALEM BAGHDAD

NUMBER 122 APRIL, 1951

Published Quarterly (February, April, October, December)
by the American Schools of Oriental Research. Editor W. F. Albright, Gilman Hall 118, Johns Hopkins University, Baltimore 18, Md. Subscription price $1.50 a year, payable to the American Schools of Oriental Research, Drawer 93A, Yale Station, New Haven, Conn.

Entered as second class matter January 18, 1934, at the Post Office at Baltimore, Md. Acceptance for mailing at special rate of postage provided for in Act of February 28, 1925.

PRINTED IN THE UNITED STATES OF AMERICA, BY J. H. FURST COMPANY, BALTIMORE, MARYLAND


IN HONOR OF

WILLIAM FOXWELL ALBRIGHT

ON HIS SIXTIETH BIRTHDAY, MAY 24, 1951

## TABLE OF CONTENTS


| | PAGE |
|---|---|
| From the President's Desk | 3 |
| The Dating of the Dead Sea Scrolls. Millar Burrows | 4 |
| Manuscripts Belong to Archaeology. Kenneth W. Clark | 7 |
| Early Egyptian Dialect Interrelationships. William F. Edgerton | 9 |
| A Preposition of Interest to Historical Geographers. H. L. Ginsberg | 12 |
| "Go View the Land." Nelson Glueck | 14 |
| The Problem of Chronology and Early Hittite History. Albrecht Goetze | 18 |
| Ancient Copper Refining. James L. Kelso | 26 |
| Sumerian Wisdom Literature: A Preliminary Survey. Samuel Noah Kramer | 28 |
| Archaeology and a Point in Hebrew Syntax. Theophile J. Meek | 31 |
| Where Did Ezekiel Receive the Call to Prophecy? Harry M. Orlinsky | 34 |
| Syrians as Pictured in the Paintings of the Theban Tombs. James B. Pritchard | 36 |
| The Candlestick Decoration on Byzantine Lamps. O. R. Sellers | 42 |
| The Date Formula of Shu-Ilishu of Isin. Francis Rue Steele. | 45 |
| An Epigraphical Expedition to North-Eastern Transjordan. F. V. Winnett | 49 |
| An Important Correlation Between the Palestinian and Syrian. Chalcolithic. G. Ernest Wright | 52 |


Guest Editor—E. A. SPEISER

W. E. B. Du Bois

## FROM THE PRESIDENT'S DESK

The American Schools of Oriental Research are rejoicing in a succession of anniversaries. The year 1950 marked the semi-centennial of their foundation and was appropriately celebrated as an occasion for retrospect in December by the Trustees, the representatives of the Corporation members, and honored guests. The occasion was made memorable by the distinguished presence of our first Director at Jerusalem, Professor Charles C. Torrey. The year 1951, it was early noted, brings the sixtieth birthday of Professor William F. Albright (May 24), who through many years of devoted service, as First Vice President, as chairman of numerous committees, as editor of the BULLETIN, and above all as Director of the Jerusalem School has contributed so much to the further development of the Schools and of their scholarly stature. This too it seemed highly fitting to celebrate in an appropriate manner.

At Professor Albright's own request there will be no encomia and eulogies. Rather the celebration is taking on a scholarly form, which is certainly appropriate, being effected in the publication of a series of articles prepared especially for the occasion and presented in the very organ, this BULLETIN, which Professor Albright has raised to such dignity by his contributions and editorship and which on this occasion is edited by Professor E. A. Speiser.

The number of those who, including the writer, might have been found willing and anxious to contribute to this Anniversary Number of the BULLETIN is as large as the circle of Professor Albright's friends. But the size of the BULLETIN itself, not to mention the budget available for its publication, imposed severe limitations. Given these limitations it was necessary to have recourse to a formal and necessarily arbitrary principle of selection: The contributors invited must have served recently in the field as leading staff members of one of the Schools. The few exceptions to this rule were calculated to insure the representation of some of the other fields of study to which Professor Albright has made outstanding contributions.

The result still leaves major gaps, some unavoidable and others unforseen. It would have been difficult in any case to reflect Professor Albright's varied scholarly interests within the limits of any single publication. What has been actually brought together is only a sampling of the tribute we would bring. It is, however, also a token of the common affection of all. In submitting it we reserve to our colleague the prerogative of commenting favorably or adversely upon material and judgments in subsequent issues of the BULLETIN, should occasion arise.

CARL H. KRAELING,
*President.*

# THE DATING OF THE DEAD SEA SCROLLS

MILLAR BURROWS

Since Professor Albright was one of the first to pronounce the Dead Sea Scrolls both authentic and ancient, it seems appropriate to dedicate to him a few remarks on this question. The subject to be discussed, be it observed, is not the date but the dating of the Scrolls. So many widely divergent views as to the date of these manuscripts have been presented that a review of the methods and presuppositions involved is in order.

A basic distinction which is obvious but has not always been kept clearly in mind is the distinction between the dates of the manuscripts themselves and the dates of composition of the writings contained in them. The books cannot have been composed after the manuscripts containing them were copied, yet some of the current theses concerning the dates of composition blithely ignore the evidence on the time when our copies were made.

For the dating of the manuscripts themselves the first available evidence was that of palaeography. It was the sole basis of the first attempts to date the manuscripts. Its adequacy is limited by the fact that the amount of definitely datable materials for comparison is not as abundant as might be wished. No contemporary Hebrew manuscripts are extant. The available specimens of Hebrew writing which can be considered contemporary with the Dead Sea Scrolls consist almost entirely of brief inscriptions or grafitti, and they cannot be dated precisely. Under these circumstances palaeography can establish only a general range of possibilities, with some elasticity at both ends. It cannot, however, be stretched too far. To allow a range of more than about two centuries is to defy definite, objective evidence. Trever, Albright, and Sukenik, who were the first to date the scrolls on this basis before any other evidence was available, agreed very closely in their conclusions, which have been signally confirmed by the archaeological evidence discovered later.

In a collection of manuscripts as extensive as that of the Dead Sea Scrolls we should expect to find, and do find, differences in palaeography. The latest manuscripts in the collection cannot be later than the *terminus ad quem* established on other grounds for the collection as a whole, but some of them may be very much earlier. The palaeography indicates that the Syrian Isaiah Scroll (DSIa) is as much as a century older than some of the other scrolls. When, as in DSIa, there are additions and corrections, the latest hand cannot be later than the time when the manuscripts were deposited in the cave where they were found.

For determining this time we are dependent upon the archaeological evidence provided by the excavation of Harding and de Vaux. This consists chiefly, but not entirely, of the criterion afforded by the jars in which the manuscripts were preserved. The fact that these jars are

all of the same type, which can be definitely dated as pre-Herodian, is the most important fact in this connection. The presence of a small amount of later Roman material, regarded by the excavators as intrusive, cannot be disregarded, but it is much less decisive for the dating of the manuscripts. It is true that we have no definite evidence as to the exact relation between the scrolls and the jars. The manuscripts cannot have been placed in the jars before the latter were made, but the ceramic evidence cannot tell us how old the manuscripts were when they were placed in the jars, nor how old the jars were when the manuscripts were placed in them. Any thesis, however, which postulates an interval of time between the manufacture of the jars and the deposit of the manuscripts in them becomes improbable in proportion to the length of the interval postulated. If the jars had been preserved and used and re-used for this purpose over a long period, one would expect to find at least a few jars of later manufacture in the collection. Some or all of the manuscripts may conceivably be later than their containers, but some or all of them may also be earlier.

Aside from the evidence provided by the excavation, scientific tests may establish the age of the leather and ink used for the manuscripts. Thus far we have very little evidence of this sort, because such tests can be made only to a limited extent without destroying the materials. For the corroboration or correction of the conclusions drawn from other evidence it is to be hoped that further tests can be made.

The language of the manuscripts affords some indication both of the original date of composition of the documents, and also of the time when the copies were made. The Book of Isaiah was written many centuries before the earliest date that can be ascribed to the Dead Sea Scrolls, but there are orthographic and even grammatical phenomena in DSIa which are peculiar to this particular copy. The same things may be true, to a lesser degree, of some of the other compositions.

Related to the question of language is that of the text. Here again it is the Book of Isaiah which is especially concerned, because we have other copies of it presenting other forms of its text. In this connection "two-way" reasoning must be avoided. One cannot assume *a priori* a date for a particular form of the text, or for a particular reading or group of readings, and then proceed to date the manuscript by comparison with this artificial standard. Most of the arguments presented thus far in this area have been based on very questionable assumptions concerning the textual history of the Old Testament. The surprising extent of agreement between DSIa and the Masoretic Text may prove only that the text had been standardized at an earlier date than we have commonly supposed.

To determine the time when each of the compositions contained in the manuscripts was originally written we must use various kinds of internal evidence. Of these the apparent historical allusions in the documents come first. The Habakkuk Commentary and the scroll of the War of the Sons of Light with the Sons of Darkness are particularly rich in such material. Unfortunately there is still room for difference of opinion as to the degree to which the persons and events mentioned

were actually historical. Even where a historical reference is virtually certain (as in the Habakkuk Commentary), or at least worthy of investigation as a working hypothesis (as in the War of the Sons of Light with the Sons of Darkness), the allusions are so indefinite as to permit more than one plausible theory. If the Kittim, for example, are not the eschatological foes of Israel, are they the Macedonians, the Romans, or the Crusaders? All these identifications have been seriously proposed. Only when the dates of the manuscripts have been established by other criteria can these allusions be definitively identified, but to the degree that any hypothesis is able to account for all of the allusions it has evidential value along with other and more objective data.

Literary relations, ideas and practices, and linguistic phenomena must all be taken into account along with the other evidence. Least reliable of all is the criterion of literary relations, because it is usually a tool that may cut either way. The occurrence of ideas, words, and linguistic peculiarities which can be fitted into a demonstrable scheme of historical development may be significant, but here as in the matter of palaeography it is important to avoid the assumption that we know more than we really do about the time at which the phenomenon in question first appeared.

The main moral of this whole story is that no kind of evidence can safely be ignored, and no one kind of evidence can be taken as conclusive in itself without reference to the implications of the other facts. It must be recognized, however, that the various types of evidence differ greatly in objectivity and in the degree to which our knowledge of them is sufficient to justify positive conclusions. Of all the evidence at our disposal in dating these Scrolls, the archaeological evidence of the pottery can be used with the greatest exactitude, with due but not excessive allowance for the uncertainties involved in the relation of the manuscripts to the jars. Paleography comes next in this respect. The historical allusions, in so far as they can be shown to be such, may be highly significant within the limits imposed by the palaeographic and ceramic evidence. Criteria of language, religious ideas, and literary relations are equally subject to the same limitations.

Observance of these points will lead to fairly definite and sure conclusions. The purpose of this paper, however, is not to draw conclusions but to reduce the prevalent confusion and promote greater agreement by recalling a few important considerations which writers on the subject have not always kept in mind. Specific references to individual scholars and their writings have been deliberately avoided; their number is already large and is rapidly increasing, but few thus far have considered thoroughly all the means and methods of dating the scrolls.

# MANUSCRIPTS BELONG TO ARCHAEOLOGY

KENNETH W. CLARK

Manuscripts are commonly found in art museums and university libraries, but seldom is one seen in an archaeological museum. In the latter are found inscriptions in stone and on clay tablets, and occasionally fragmentary texts on papyrus. But rarely does one find there codices on papyrus and parchment and paper—such as we usually designate "manuscripts." Furthermore, while the archaeologist has undertaken to dig up the remains of the past, manuscripts produced and used in medieval centuries have not been included in his objective. Manuscripts are thought to lie in a separate category of research, and to be the responsibility of a separate group of scholars not usually classed as archaeologists. It would have important effects upon manuscript discovery and research to recognize that manuscripts belong to archaeology.

Perhaps the popular distinction between archaeology and manuscripts derives from the circumstance that manuscripts are not often dug up. It is commonly assumed that archaeology implies excavation, but the two are not inseparable. Eighty-five years ago a number of manuscripts were dug up in North Carolina. They had been but recently buried by the owner when Sherman's army was approaching. Although they were antiquities dug out of the earth, this did not constitute an archaeological excavation. The converse of this may be urged with equal cogency, that the discovery of manuscripts belongs to archaeology even though they may not be dug out of the earth.

It is relevant to recall that many archaeological finds were never exhumed, simply because they were never completely buried. Archaeology possesses the pyramids and the sphinx, the tall columns of Baalbek and Jerash and of temples in Athens and Rome—never lost from the sight of man. It is true that earth has been removed to clear and reveal more at these sites, but no one will contend that archaeology begins only where the digging starts. Certainly those other sites which time has buried are no more archaeological than the monumental structures which have always stood forth. This serves as a reminder that excavation is not the mark of archaeology, but rather the frequent servant in the discovery and examination of witnesses to ancient life and times.

Neither is it possible to maintain the view that archaeology is nonliterary, dealing only with mute artifacts less expressive than literary witnesses. No one questions that it was archaeology that brought forth the Pyramid Texts, or the Hammurabi Stele, or the Rosetta Stone, or the Moabite Stone. From these it is a short step to the tablets of Tell el-Amarna and Ras Shamra and Boghazköy. It was archaeology that discovered also papyri stuffed in crocodiles or wrapped around mummies, or even more accessible in baskets under the sand. It is not basically different when the scholar acquires papyrus or leather documents found

by natives in a cemetery or a cave. Although the archaeologist would prefer to be first at the site, it is often necessary to take over wherever the trail is picked up. The objects discovered and their decipherment or interpretation are no less archaeological. It is also the province of archaeology to discover later parchment scrolls and codices, manuscripts that have lain for centuries in isolated monasteries or in private homesteads. Although they were never buried, and may even have been seen and listed, they lie like many a known archaeological site still unexposed and unexplored. All these forms of ancient literary documents, inscribed by the hand of man, belong to archaeology.

It is not reasonable to consider as archaeological only such literary discoveries as are inscribed in stone or baked in clay. The province of archaeology includes leather documents like the scrolls from the Dead Sea cave, and papyrus sheets from graves and rubbish heaps. But it also includes papyrus and parchment and paper documents, in roll or codex, though they never have been buried in the earth. From stone to paper, the texts recorded thereon by the hand of man all belong to archaeology.

It may well be pointed out that archaeology deals with antiquities, and time draws a limit for its concern. This is surely true, though archaeologists may differ as to the limit each would draw. This limit of concern is an individual matter, and depends to a great extent on the area of research in which one is interested. But archaeology itself is a discipline broad enough to embrace a wide range of interests and a long reach of time. It is evident that a broadening interest has emphasized the New Testament and the Roman periods, and the Arabic and Turkish periods. Many an archaeological museum has declared its province to include the seventeenth century, or even beyond. Certainly the printing press had already arrived before the *ad quem* date that a typical museum should set as the limit to its interest in antiquity. The majority of manuscripts, and certainly the bulk of all important manuscripts, were written by the end of the fifteenth century when the printed book emerged from its cradle. Relatively few books were hand-copied after the seventeenth century, and seldom a significant one. If all these factors resolve in a logical conclusion, it is that archaeological institutions should actively embrace the field of manuscript research as an integral part of their responsibility and service.

As a subsidiary discipline chirographology has contributed much to the effectiveness of archaeology. Manuscripts and other artifacts go hand in hand. The former often speak where the latter are inarticulate. Indeed, the manuscript often guides the archaeologist to the discovery of those sites and objects which will illustrate and illuminate the text. Because many an ancient text has been printed, it is easy to fall under the delusion that the text itself is no part of archaeology. We need only the reminder that every ancient document demands a critical text which, in turn, requires that manuscript copies be uncovered and thoroughly explored. It is well to recognize that these manuscripts are *desiderata* of archaeology.

The particular manner in which a manuscript may come to light is incidental to its archaeological nature. The Herculaneum Rolls were

salvaged from a volcanic flow. The Oxyrhynchus Papyri were dug out of the sand. The Beatty Papyri were probably taken from a grave. The Dead Sea Scrolls were drawn from a cave. Codex Sinaiticus was brought out from a monastery. Codex Vaticanus was unearthed in its own library. Yet each instance constitutes an archaeological episode.

Most manuscripts never have been buried under the earth. But they require an archaeological expedition to disinter them from obscurity. Enormous tasks remain if we would explore the chirographic sites of which we know. To date, a few hundred manuscripts have been consulted, whereas thousands have been catalogued. Though thousands have been catalogued, many more thousands have not been even listed. These thousands of manuscripts still lie buried, figuratively. We know a "tell" is here, or there, but each awaits the expedition that will release these witnesses of the past and give them tongue.

Until 1950, only minor and individual efforts have been made to exploit the wealth of manuscripts extant. Textual studies have made notable progress in the last two hundred years, but when this period is seen in perspective from a future vantage point it will be clear that only a pioneer stage has so far taken place. We have arrived at the beginning of a new era, when archaeological institutions recognize manuscripts as within their sphere. This recognition has already come from such significant institutions as the American Schools of Oriental Research and the Oriental Institute. Their new activities and plans in this respect have had the support and collaboration of William F. Albright, the "dean" of America's Near Eastern archaeologists, who stands in the vanguard of this development.

Through such agencies alone it becomes possible to "excavate" manuscripts on a large scale, at great chirographic sites like St. Catherine's Monastery and the Greek Patriarchal Library in Jerusalem. The Near East offers scores of collections like these, and to many such sites the archaeological program of university and theological school and museum may be directed. We can no longer be content with a slow man-by-man progress. No archaeological site can be satisfactorily interpreted by the occasional discovery of an inscription or a jar or a coin. Only an organized expedition with proper equipment and resources is adequate. So will it be with manuscripts, since we have come to apply the archaeological technique. Chirographology has come of age with the recognition that manuscripts belong to archaeology.

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## EARLY EGYPTIAN DIALECT INTERRELATIONSHIPS

William F. Edgerton

Since Old, Middle, and Late Egyptian succeeded one another chronologically in that order, it is natural to assume that Middle Egyptian is derived from Old Egyptian and Late from Middle Egyptian. As a working hypothesis, this probably underlies almost all of the discoveries which

have ever been made in the subject, and its utility is by no means exhausted. But there are reasons for suspecting that the actual genetic relationship may have been somewhat more complex.

There are a few phenomena which belong to standard usage in the Old Egyptian Pyramid Texts and again in Late Egyptian, but not in Middle Egyptian:

I. The prothetic syllable of certain verb forms, including the imperatives of biconsonantal verbs and certain participles of the same verbs. For instance, the common imperatives *i-ḏd* "say!," *i-šm* "go!," and *i-wn* "open!" show the prothetic syllable both in the Pyramid Texts and in Late Egyptian, but not in Middle Egyptian.[1] The spelling with the prothetic group is the typical one for biconsonantal imperatives generally in Old and Late Egyptian. In Middle Egyptian it seems to be limited to *i-sy* "go!" and *i-ms* "bring!"[2]

II. The sentence consisting of two nouns without copula.[3] For instance, *bwt NN ḫs* (Old Egyptian), "NN's abomination (is) excrement," and *p3 bt3 n p3y sh 2 n p3 ḫr p3y=w ph p3y ḫ3ty-ʿ n Niwt* (Late Egyptian), "The crime of these two scribes of the Necropolis (was) their going to this mayor of No." Such sentences are freely used in Old and Late Egyptian; in Middle Egyptian they occur only in personal names (especially royal names) and in statements that a person's name is So-and-so or that his father (or another relative) is So-and-so.

III. The sentence consisting of noun or pronoun predicate followed by a demonstrative pronoun subject which is either masculine singular (OEg *pw, py,* or *p*; LEg *p3y*), feminine singular (OEg *tw* or *ty*; LEg *t3y*), or common plural (OEg *nw*; LEg *n3* [or *n3y?*]) to agree with the antecedent. Old Egyptian examples[4] are *d3ng pw* "He (is) a dwarf," *'Ist tw ḥnʿ Nbt-ḥt* "that (fem. sing.) (is) Isis, together with Nephthys," and *ṯs 2 nw n(y)w 3bw* "those (are) the two knots of Elephantine." Late Egyptian examples[5] are *ṯsm p3y* "that (masc.) (is) a grayhound (masc.)," *Rʿ t3y* "she (the deceased woman) (is) Re," and *nfrw ḫʿ n3* "these (are) the benefactions (?) of a lifetime (?)."[6] In Middle Egyptian, all sentences of this type use the masculine singular demonstrative *pw* as subject, regardless of the gender or number of any other element in the sentence; this construction with invariable demonstrative subject is also found in the Pyramid Texts[7] but not in Late Egyptian.

Each of the three phenomena just mentioned is a prominent feature of

[1] Sethe, *Verbum*, II, §514.

[2] *'I-rḫ* "inquire!" cited by Gardiner, *Grammar* (1927), §336 from ÄZ LVII (1922), p. 104, stands in copies of an Old Egyptian text, "ein Zeitgenosse der Pyramidentexte, ja sogar ihrer älteren Fassungen" (Kees, ÄZ LVII, p. 96).

[3] Sethe, *Nominalsatz*, §§24-29.

[4] Drawn from *Nominalsatz*, §89.

[5] The first and second are from *Nominalsatz*, §96.

[6] Quoted by Behnk, *Grammatik der Texte aus el Amarna* (1930), §98, from Davies, *The Rock Tombs of El Amarna*, VI, pl. 30.

[7] *Nominalsatz*, §90.

the oldest Egyptian known to us, and again of Late Egyptian. No one of the three seems to be a living element in Middle Egyptian. If Old, Middle, and Late Egyptian represent successive stages in the development of a single dialect, then three apparently unrelated changes must have occurred between Old and Middle Egyptian and these same three changes must have been exactly [8] reversed between Middle and Late Egyptian. Doubtless that is possible, but it is also possible that Old and Late Egyptian represent the speech of adjacent parts of Egypt while Middle Egyptian may represent a different and perhaps distant part.

Some parts of the Pyramid Texts were composed before the beginning of the First Dynasty, and in different places (some, for instance, in Heliopolis, some in Buto, some in Upper Egypt) but our oldest copies were carved on stone at Saqqara toward the end of the Old Kingdom and some parts were relatively new at that time. I have not tried to determine whether the three phenomena mentioned above belong to one part of the Pyramid Texts rather than another. It suffices, for my present purpose, that the phenomena are characteristic of Old Egyptian and not of Middle Egyptian.

Middle Egyptian became the dominant literary dialect during the political and social convulsions of the First Intermediate Period. It began to show traces of Late Egyptian influence during the Second Intermediate Period (the epoch of the Hyksos). Late Egyptian was sharply thrust into prominence by the Amarna revolution, though the royal inscriptions of the Nineteenth and Twentieth Dynasties still show much Middle Egyptian influence.

Late Egyptian is generally, and I think rightly, regarded as the direct ancestor of Demotic and Coptic. The relationship of Middle Egyptian to Late Egyptian is not so clear to me. I consider it very possible that Middle and Late Egyptian may represent different parts of Egypt as well as different centuries. In any case, there was of course constant contact among all communities in Egypt. Especially when the land was politically united, the king's vast army of servants, both high and lowly, traveled from end to end of the kingdom. Even in anarchy and disunion, the servants of the great gods pursued the business of their temples far and wide. It may reasonably be assumed that all native dialects in the country were mutually intelligible at all times, in spite of some difficulties. Mutual influence, therefore, would never be wholly absent. At different times, now one dialect and now another would enjoy superior prestige. The breaks which we observe in the history of the language may be due not only to the passage of time, but also perhaps in part to the seizure of power, wealth, and prestige by successive groups who came, by and large, from different areas. Standard Old Egyptian was perhaps

[8] The prothetic syllable is extended analogically in Late Egyptian to some forms which could not take it in Old Egyptian, e.g., *i-sdm=y* "which I heard" and *i-sdm=f* "which he heard" in Twentieth Dynasty administrative documents (*Verbum*, II, §795). Such forms from a triconsonantal verb would be unthinkable in Old Egyptian. But these changes cannot hide the essential identity of the phenomena under discussion in Old and Late Egyptian, nor the contrast to Middle Egyptian.

based on the local speech of the region of the pyramids. The more archaic features of the Pyramid Texts may come largely from the Delta or from Heliopolis. Standard Middle Egyptian perhaps belongs to a more southerly locality (Heracleopolis? Thebes?). Late Egyptian again may have come from the Delta or near it.

What is offered above is no more than a working hypothesis. Its chief interest, so far as the history of the Egyptian language is concerned, is that if the hypothesis is correct then what I have called Paleo-Coptic [9] probably did not correspond precisely to any form of Egyptian recorded in the documents now known to us.[10]

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## A PREPOSITION OF INTEREST TO HISTORICAL GEOGRAPHERS

H. L. Ginsberg

For a period of no less than fifteen years I have felt that in a number of passages in the Pentateuch and the Former Prophets the preposition *'aḏ* does not indicate direction at all, but only position. Since this is notoriously often true of Latin *ad*, and other analogies can be cited, the Hebrew *'aḏ* " at, by, near " is not necessarily etymologically distinct from *'aḏ* " until, as far as." Nevertheless, I have often wondered if the former is not connected with Arabic *'inda* (dialectal *'and*); so that it assumed the form *'add-* if and when it took the pronominal suffixes, and lacked the poetic by-form *'dē*. But he that as it may, the positional force of *'aḏ* in the passages I have in mind is so pronounced that I can only marvel that every dictionary, translation, and commentary I have consulted is content to make shift with the ordinary, directional, sense of the word.

Surely, all that the last clause in Gen. 13:12 tells about Lot is that " he tented *near* Sodom," and all Jud. 4:11 b wishes to say about Heber the Kenite is that " he pitched his tent *at* Elon-bezaanannim (or, *by* the holy tree of Bezaanannim) which is near Kadesh." By the same token, all that the relative clause in Deut. 2:23a conveyed to an ancient Hebrew was that the Avvim had " dwelt in villages *by* (or *near*) Gaza."

Let us take a closer look at the first two of the above passages. The information that Lot tented *'aḏ* Sodom is preceded (Gen. 13:11) by a report that he separated himself (nifal of *prd*) from his previous neighbor. It is the same, *mutatis mutandis*, with Heber the Kenite: first the nifal of *prd* is predicated of him in Jud. 4:11a, then we are told in v. 11b *'aḏ* what he pitched his tent (*wyṭ 'hlw*, reproducing only the letters of the Hebrew text in this and in most of the following examples) after that. In the light of these observations let us examine Gen. 38:1. Surely it is obvious (1) that *wyrd* means very much the same thing as *wyprd*, and that, as Ehrlich has already conjectured, the former is prob-

[9] JNES VI (1947), pp. 1-17.

[10] This possibility was briefly alluded to, JNES, p. 3, n. 6.

ably a corruption of the latter; but also (2) that *wyṭ 'd* here is simply an ellipsis for the *wyṭ 'hlw 'd* of Jud. 4:11b, so that Gen. 38:1b is to be translated: "and pitched his tent *near* an Adullamite man by the name of Hirah." (On *wy'hl* in Gen. 13:18 see Ehrlich.)

It should next become obvious that in 10:19, both in the a-half and in the b-half of the verse, only the expression *bō'ḵā* means "all the way to"; whereas *'aḏ* in both cases means "by." Although vv. 15-18 imply that "Greater Canaan" extended quite a distance north of Sidon, our verse fixes the northern end of "Canaan proper" at Sidon. Since at the latitude of that city the strip of anciently Canaanite territory is very narrow, being hemmed in between the Mediterranean Sea and the Lebanon Mountains, there is no need for indicating its width by naming an inland town in addition. At the southern end of the Canaanite area, however, it extended right across the low hill country to the Dead Sea Depression (though according to 13:12 it did not include it); and besides, the biblical writer doubtless knew this end much better. Accordingly, our verse (Gen. 10:19) is not content to name only one point at the southern end, but defines both the southwestern and the southeastern corner of the Canaanite land. On the southwest the latter is said to have extended to Gerar; whose position, because it is a small place, is defined by its proximity to a larger one: "all the way to Gerar, *near* Gaza." The other corner used to be marked by the four cities of Sodom, Gomorrah, Admah, and Zeboim; which, however, are but a legend in the author's time. He therefore has to locate them for his readers by stating what extant city they lay near: they were '*near* ⌜Bela⌝.' We know from Gen. 14:2, 8 that Bela was the ancient name of Zoar, and both from these verses and from 19:20-22 that this city was a neighbor of the other four. Kittel (*BH*[2] and *BH*[3]) was surely right in proposing the emendation *bl'* for *lš'* in Gen. 10:19. The *b* was lost by haplography after the preceding *d*, and the *š* is due to contamination by the sequence *lš* in *llšntm*, v. 20. Even Kittel, however, does not seem to have realized the true force of the preposition *'aḏ* in this verse. (That this obviously very ancient notice should employ the archaic name of Zoar does not constitute a difficulty.)

Again, in Gen. 12:6 it is surely only the first *'aḏ* that means "as far as." The second merely locates 'the (holy) place of Shechem' (not the city itself) "*by* the sacred tree of Moreh." (For *māqōm* 'holy place' cf. Gen. 22:3 f.; 28:11, 19; etc.) The tree in question may have been a very famous landmark, perhaps identical with that of Gen. 35:4; Josh. 24:26, Jud. 9:6; and/or Jud. 9:37. Or alternatively, all these sacred trees of Shechem were distinct and in part co-existent, each with its own *māqōm*, so that the *māqōm* of Gen. 12:6 had to be identified by the particular sacred tree with which it was associated.[1]

[1] Cassuto, *A Commentary on the Book of Genesis, Part II: From Noah to Abraham* (Jerusalem 1949, Hebrew), p. 119, rightly stresses that (*'aḏ*) *bō'ḵā* means everywhere (see particularly Gen. 13:10; Jud. 6:4; 1 Kings 18:46) "all the way to" (literally "*until* [not *as*] *thou comest to*"). To be sure, even a border which extends all the way to a certain locality does not necessarily include it; but Cassuto, taking the following *'aḏ* in the traditional sense of "until," claims that the intention

Further, since *'šwr* as a geographical expression never means anything but "Assyria" (or, in late passages, "Mesopotamia"), it is best, assuming that the text of Gen. 25:18 is in order, to understand "Assyria" here too (rather than the land of the Asshurim of v. 3). Gen. 25:18a will then have to be rendered thus: "And they (Ishmael's descendants) settled from Havilah, by Shur which is over against Egypt, all the way to Assyria." Especially if the Ishmaelides in a wider sense embraced the descendants of Keturah, the preceding sentence states no more than the truth; inasmuch as Kedem (see v. 6) extended all the way to the Middle Euphrates. To convince oneself of that, one has only to compare Gen. 29:1 with Gen. 29:4 and Num. 22:5 with Num. 23:7; and besides, the Shuah of Gen. 25:2 has been connected with cuneiform *Sûḫu*, near where the Balikh flows into the Euphrates, even as the Pethor of the aforementioned verse Num. 22:5 has been identified with cuneiform *Pitru*, just south of Carchemish. Entirely in keeping with this interpretation of Gen. 25:18a will be 25:18b: "He (Ishmael) encamped over against all his brethren." [2]

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## "GO, VIEW THE LAND"

Nelson Glueck

Between the years 1932 and 1947, the expeditions of the American Schools of Oriental Research examined the surface remains of over a thousand antiquity sites throughout Transjordan, from the Gulf of 'Aqabah to the Wâdī Yarmûk and from the Jordan river to the Syrian and Sa'ûdī Arabian boundaries in the eastern desert. The conclusions arrived at [1] were based in the first instance on surface pottery finds, the earliest of which date back to the neolithic period.

The scientific knowledge of ancient pottery, including surface sherds, as developed in Palestine particularly by Sir Flinders Petrie, Père Hugues Vincent, Clarence Fisher, and in extraordinary measure by William Foxwell Albright, is an absolute prerequisite for archaeological exploration of Bible lands and places. Ancient pottery speaks with muted eloquence of long perished and buried cities and settlements. Examination even of surface potsherds on such sites, for instance, as Tell Nimrîn and Tell er-Râmeh in the Plains of Moab, reveals that they

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of Gen. 10:19 is to include Gerar and Sodom and its sisters within the borders of Canaan and to continue them beyond these points down to Gaza in the one case and "Lasha" in the other. However, my interpretation of *'aḏ* here as a preposition expressing not direction but proximity renders the inclusive interpretation of *bō'ḵā* unnecessary, and other considerations render it improbable.

[2] The above may have some bearing on two passages in First Samuel. (1) Since Havilah appears from the foregoing to have been situated in the vicinity of Shur, the possibility that the reading *ḥwylh* (rather than Wellhausen's *ṭylm*) may be right after all in 1 Sam. 15:7 should be considered anew. (2) Perhaps, too, in 1 Sam. 27:8 *w'aḏ* (in spite of the copula) again means simply "near" or "by."

[1] Cf. *Annuals* XIV, XV, XVIII-XIX, and XXV-XXVIII (in press).

were first settled in the Hellenistic-Roman period, long after the Biblical cities of Beth-nimrah and Beth-harâm, with which they have been identified because of clear relationship of names, had been destroyed and abandoned. The Biblical names had wandered from their original sites nearby to new locations.[2]

Despite certain lacunae, the archaeological exploration of Transjordan may now be considered as being fairly complete. Based on the technique of pottery identification, it represents the continuation of Albright's discoveries of some of the Bronze Age cities lastingly destroyed by the Eastern Kings mentioned in Genesis 14,[3] as well as of his other explorations and excavations in Transjordan.[4] Uniform results have been achieved by all scholars who have undertaken archaeological exploration in Transjordan in modern times.[5] It is possible that these conclusions may have to be modified to some degree if extensive excavations are ever carried out particularly in Bronze Age sites there.

The extension of the archaeological survey by the American Schools of Oriental Research into North Gilead[6] and the Jordan Valley, with well over three hundred sites being carefully examined in those areas, has produced much new information. The rise and fall of advanced civilizations, with eras of decline or emptiness in between, were documented, as they had been through earlier explorations in the other parts of Transjordan. Widespread archaeological evidence has shown that after periods of growth, these urban civilizations had failed and fallen not because of climatic changes but rather because of radical disturbances in public security.

Excavations at Jericho and surface pottery finds elsewhere in the Jordan Valley reveal the presence of Neolithic civilization there. It was followed by the Chalcolithic civilization, whose entire course can be traced in the Jordan Valley and can be largely paralleled in the hill country and plains of Transjordan and Palestine. There seems to be a considerable time gap between the Neolithic level of Jericho IX and the Early Chalcolithic level of Jericho VIII.[7] After Jericho VIII and other Early Chalcolithic sites such as Megiddo XX, pits below Bethshân XVIII, Teleilât Ghassûl II (and III?), Khirbet 'Ain Dûq, Tell es-Sa'îdiyeh, Abū Naml by Sha'ar hag-Gōlân, some of the earliest settle-

[2] Cf. BULLETIN 91, pp. 11-26.

[3] Cf. *Annual* XVIII-XIX, p. 265, and references there.

[4] Cf. Albright, *The Archaeology of Palestine and the Bible*, ed. 3, (APB), pp. 136-137. 142; BULLETIN 14, p. 10; 53, pp. 13-18; 95, pp. 3-13.

[5] Cf. *Palestine Exploration Fund Quarterly Statement* 1934, pp. 76-84; *Annual* XIV, pp. 55-56; *Revue Biblique* (RB) 47, pp. 404-421; *Bulletin of the Jewish Palestine Exploration Society* (BJPES) X:4, pp. 101-102; BULLETIN 90, p. 17, n. 77a; Albright, *The Archaeology of Palestine*, (AP), pp. 82-83.

[6] Cf. *Annual* XVIII-XIX, p. 153.

[7] Cf. Wright, *The Pottery of Palestine from the Earliest Times to the End of the Early Bronze Age* (PPEB), p. 27; Albright, AP, p. 66; *The American Journal of Archaeology* LIII:2, 1949, p. 214; BULLETIN 86, p. 11; 89, p. 25; 114, p. 18; *Journal* of the American Oriental Society (JAOS) 70:1, 1950, p. 57; *Biblica* 28, 1947, p. 310; *Annals of Archaeology and Anthropology of the U. of Liverpool* (AAA) 1937, pp. 35-51; *Teleilat Ghassul* (TG) I, p. 115, Fig. 60:3. 4; *The Museum Journal*, 1935, *Beth-Shan*, p. 7.

ments in the Wâdī Ghazzeh, the "Énéolithique Moyen" of Tell el-Far'ah near Nablus, and the Mughâret Abū Usba' deposit with "herringbone" pottery, came the Middle Chalcolithic period of Teleilât Ghassûl IV and of 'Affûleh and of Khuḍeirah.[8] The subsequent Late Chalcolithic period is represented in the Jordan Valley by the earliest phase of Tell Umm Ḥamâd Sherqī,[9] and by Beth-shân XVIII-XVI, and is paralleled by the Esdraelon phases of Megiddo XIX and 'Affûleh and by the "Énéolithique Supérieur" of Tell el-Far'ah.[10] Some clear cut Middle Chalcolithic sites, including Tell el-Meghâniyeh, Umm Beṭeimeh, Saḥrī, and Zakhîreh have been discovered by the American Schools of Oriental Research expeditions in the hill country of North Gilead.[11] There is reason to believe that such Chalcolithic sites existed also in the rest of the cultivable hill country of Transjordan.[12]

After a period of transition from the Chalcolithic period, there developed a distinctive Early Bronze Age civilization both in the Jordan Valley and North Gilead as well as in the areas south of the Wâdī Zerqā.[13] It is much better preserved in North than in Central and South Transjordan. Although the last part of the Early Bronze Age, called EB IV and dating roughly from the 23rd to the 21st centuries B. C., is well represented in Transjordan south of the Wâdī Zerqā,[14] there seems to have been a marked decline, amounting almost to a complete break in the history of urban occupation during that period in the Jordan Valley and in North Gilead. Although, for instance, EB I-III are well attested to both by surface finds and excavation results at Khirbet Kerak (Beth-yeraḥ) on the Lake of Galilee, it is noteworthy that no evidence of an EB IV settlement has been found there.[15] The EB III period at Khirbet Kerak was a flourishing one, distinguished by the lustrous red and black burnished "Khirbet Kerak" ware.[16]

Both in North Gilead and in the Jordan Valley, there was a strong revival of sedentary civilization during the MB I period, with many important MB I sites being established completely anew on virgin soil. This fact presupposes a period of weakening or collapse of security and of most settlements extending through the preceding EB IV period. The MB I civilization was a highly developed one, with the finest types of the distinctive and excellent MB I pottery present in abundance on numerous sites throughout North Gilead and the Jordan Valley.[17] MB I pottery was often found together with EB IV pottery in Transjordan south of the Wâdī Zerqā, and but infrequently together in North Gilead

[8] Sukenik, *Archaeological Investigations at Affula* (SAIA), pp. 18-20; *Journal of the Palestine Oriental Society* (JPOS), XVII, 1937, pp. 15-30; AP, pp. 68. 69.

[9] BULLETIN 97, pp. 10-12.

[10] Cf. RB LIV, 1947, pp. 397-403. 406-409; LV, 1948, pp. 545-548. 558-559; LVI, 1949, pp. 103-109. 113. 114; AJA LIII:2, 1949, p. 214; AP, p. 70.

[11] Cf. BULLETIN 104, pp. 12-20.

[12] Cf. BULLETIN 104, p. 16.

[13] Cf. *Annual* XVIII-XIX, p. 268; AP, p. 77.

[14] Cf. *Annual* XVIII-XIX, pp. 255-256. 257-262.

[15] Cf. BJPES X:4, pp. 98-102; XI:1, pp. 17-21; AP, pp. 48. 74.

[16] Cf. the Khirbet Kerak pot from 'Affûleh in Glueck, *The River Jordan*, p. 194, Fig. 92, erroneously ascribed there to Beth-shân; Y. Sukenik, BULLETIN 106, p. 12.

[17] Cf. BULLETIN 100, pp. 7-8.

or in the Jordan Valley. It seems that this sudden appearance of an advanced MB I civilization in North Gilead and the Jordan Valley may possibly represent not only a strong cultural influence but aslo a pronounced population influx from Syria,[18] bringing with it the skills necessary to produce the well levigated, hard baked MB I pottery, whose necks and rims were shaped on the potter's wheel.

Following the MB I period, there seems to have set in a sharp decline of permanent settlement and an accompanying centralization of sedentary population at fixed and strongly fortified points during most of the MB II and at least the first half of the subsequent LB period. In North Gilead and in considerable parts of the Jordan Valley, this marked decline seems to have taken place mainly between the middle of the 18th and the 15th centuries B. C., while in the rest of Transjordan south of the Wâdī Zerqā it seems generally to have extended between the 20th and 13th centuries B.C. It is important, however, to emphasize in this connection that, although sharply limited in number, some urban settlements dependent upon an agricultural economy continued throughout the Middle and Late Bronze periods in Transjordan and in the Jordan Valley. Such a center and stronghold as Jericho was definitely occupied during the entire Middle Bronze II period, as were other important sites in the Jordan Valley during all or parts of that period and of the subsequent Late Bronze period.[19] The discovery of Middle Bronze II pottery at 'Ammân or Na'ûr or Heraqlā or of some Late Bronze tomb groups in Transjordan, merely emphasizes, as Albright has pointed out in this connection, " that occupation became concentrated in fortified towns and castles during the Hyksos period instead of being distributed through unwalled settlements." [20] The phenomenon of this severe shrinking amounts in large stretches almost to disappearance of sedentary occupation in most of the broken, fertile hill country of Transjordan between the 20th and the 13th centuries B. C., and within a narrower limit in North Gilead and the Jordan Valley. It manifested itself in the methodical exploration of all of Transjordan by the American Schools of Oriental Research, and it has been confirmed also by the archaeological survey of the es-Salṭ region by Père de Vaux and Père Benoit, by Albright's excavations at Ader and Crowfoot's at Bālû'ah, and by the significant silence of the Amarna tablets and of the Egyptian lists of conquered towns and of actual or potential rebels concerning the areas and periods in question.[21]

There is thus in Transjordan, north as well as south of the Wâdī Zerqā, a general absence of artificial city hills, *tulûl*, because of the greatly diminished number of villages and cities which were built or could be maintained during MB II and LB. This is the case also, with certain notable exceptions, in northernmost Transjordan and in the

[18] Cf. AP, p. 80.

[19] Cf. AP, pp. 83-87; BULLETIN 83, p. 33; 89, p. 9; 90, pp. 17-18, n. 77a; *Annual* XIV, p. 82; XV, p. 138.

[20] BULLETIN 90, p. 17, n. 77a.

[21] Cf. *Annual* XVIII-XIX, pp. 265. 266. 268; BULLETIN 95, p. 6, n. 8; AP, pp. 79. 82-83.186-187.

Jordan Valley. The important and extensive site of Tell Umm Ḥamâd in the Jordan Valley furnishes a striking example of why, despite the word "*tell*" which figures in its name, it in no wise partakes of the characteristics of a *tell*.[22] Were it not for the masses of fragments of pottery on its surface, this great double site would hardly be recognized as an anciently inhabited one, different from any of the low, natural rises in the neighborhood. One part of Tell Umm Ḥamâd was inhabited during the Late Chalcolithic and Early Bronze I periods between approximately 3400 and 3000 B. C., after which it was not reoccupied until the beginning of the Iron Age. Another part was settled during the Middle Bronze I period between approximately 2100 and 1900 B. C., and was never built on thereafter. No artificial city hill could develop under such circumstances, because a *tell* is formed only when a series of cities is built, each new one on the ruins of its predecessor.

During Iron Age I-II, there was intensive and widespread building of fortified cities and open settlements both in Transjordan and the Jordan Valley. With the destruction of the Iron Age II civilization in the early 6th century B. C. there ensued again a long stretch of years marked by the decline and general disappearance of cities and villages. This was followed in time, in accordance with the pattern of the now familiar cycle, by the numerous and frequently grandiose constructions of the Hellenistic-Roman and Byzantine periods. And so the history of civilization in Transjordan and the Jordan Valley from earliest historic times on can be reconstructed in considerable part from archaeological remains. It is a record marked by alternating periods of tranquility and terror, by a variation of the encroachments of the Desert and the inroads of anarchy with the flowering of the Sown and the advances of civilized order.

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## THE PROBLEM OF CHRONOLOGY AND EARLY HITTITE HISTORY

Albrecht Goetze

The historian who wishes to see the cultural development in the Ancient Near East as a whole cannot help but concern himself with chronological questions. He will by necessity try to establish the chronology of the various cultural provinces within the wider area and to co-ordinate his results. Universal as his aims may be, he will have to start out from very specific details. Today we are witnessing a new period of chronological integration. Syrian mounds have begun to yield to the excavators objects of Egyptian, Mesopotamian, and Anatolian origin in conjunction and thereby make it possible to correlate with one another the chronological systems established for the individual subareas.

The many-sided interests which we admire in W. F. Albright have

[22] Cf. Bulletin 100, pp. 7-8.

almost inevitably led him to assume a leading role in this kind of research. Flexible and open-minded as he has always proved to be, he will welcome a contribution to the chronological problem, even if it contradicts some of the opinions which he himself advanced. The problem will here be approached from the Hittite angle to which little attention has so far been paid.

* * *

Hittite chronology depends in important details on the chronology of the other parts of the Near East. Nevertheless, Hittite history can make important contributions to the wider problem which concerns the area as a whole. The present tendency is to shorten the period of time allotted to the Pre-Armana age. There is no doubt that a revision downward is indicated, but it seems to me that some of the solutions proposed shorten so excessively that it is no longer possible to accommodate the facts of which we have knowledge. This is particularly true of Hittite history between the two important events by which it is linked to the rest of the Near East. The two events alluded to are:

(I) Muršili's raid on Babylon which put an end to the dynasty of Hammurapi, and

(II) the renascence of Hittite power under the predecessors of Šuppiluliumaš.

To begin with (II) it is agreed that it must fall after the great Asiatic campaign of Thutmosis III. The Hittites could expand into Syria only in a period of Egyptian weakness. The absolute date is roughly 1450 B. C.[1] The date for (I) had in the past been assumed to lie at around 1750.[2] As a consequence of recent discoveries at Mari and in northern Syria, a lowering of this date has become necessary.[3] Some scholars may maintain that the problem is solved; it seems to me that the question as to how much of a reduction must be made, is still open. The solutions at present discussed, which owe their precision to an adjustment to the dates allowed by the Venus tablets of Ammiṣaduqa,[3a] are in the main the following four:

(a) Sidersky, Thureau-Dangin: 1651 B. C.[4]

[1] Cf., e. g., Albright, BULLETIN 118 19: "1457 at latest."

[2] E. F. Weidner, *MVAG* 20/4 (1915) 24 f.; 26/2 (1921) 41; F. X. Kugler, *Von Moses bis Paulus* (1923) 497 ff.; *Sternkunde und Sterndienst* 2 (1924) 563 ff.; Ed. Meyer, *Die ältere Chronologie Babyloniens, Assyriens und Ägyptens* (1925) 2 ff.

[3] The important points are these: (1) Hammurapi of Babylonia is a younger contemporary of Šamši-Adad of Assyria; (2) the Egyptian 12th Dynasty precedes the Mari age.

[3a] Langdon-Fotheringham-Schorr, *The Venus Tablets of Ammizaduga* (1928); S. Smith (and J. W. S. Sewell), *Alalakh and Chronology* (1940) 25 ff.; see also O. Neugebauer, *OLZ* 32 (1929) 913 ff.; *OLZ* 42 (1939) 407 ff.; *JAOS* 61 (1941) 58 ff.

[4] D. Sidersky, *RA* 37 (1940) 45-54; F. Thureau-Dangin, *Mémoires de l'Académie des Inscriptions* 43 (1942) 1.

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|---|---|---|
| (b) Ungnad, S. Smith: | | 1595 B. C.[5] |
| (c) Albright: | ca. | 1550 B. C.[6] |
| Cornelius, van der Waerden: | | 1531 B. C.[7] |
| (d) Boehl, Schubert: | ca. | 1500 B. C.[8] |

As far as Hittite history is concerned, all events between Muršili's raid on Babylon and the Syrian undertakings of Tudḫaliyaš I, Šuppiluliuma's great-grandfather,[9] would fall between this date and ca. 1450 B. C., i. e. they would fill about 200, 150, 100 or 50 years respectively depending on the solution which is accepted as true.

The known events may first be briefly enumerated here, arranged according to the successive kings; later on, under corresponding numbers, the available evidence will be quoted.

(1) Muršiliš sacks Babylon, returns to Anatolia, and is later[10] murdered in a revolution.

(2) Ḫantiliš, husband of Muršili's sister Ḫarapšiliš, reigned for a fairly long time into his old age. He had trouble not only with the Ḫurrians, but also with the Kaškeans. Although the Kaškean difficulties are not mentioned in the so-called Telepinuš text,[11] they are amply attested and at least as significant. The Kaškean attacks were so severe that Nerik, an important cult center, was lost; it was not recovered before the time of Ḫattušiliš, the grandson of Šuppiluliumaš. Ḫantiliš had to fortify Ḫattušaš, till that time an open city, and strongholds had to be erected all over the country.

(3) Zidantaš, Ḫantili's successor, was married to the old king's daughter [. . . . . .]taš (or -šaš). He was assassinated.

(4) During the reign of Ammunaš, the son of Zidantaš, the country went through a period of distress. He too was murdered.

(5) Of the following king, Ḫuzziyaš, nothing much is known, not even in what way he was related to the royal house.

(6) His successor, Telepinuš, husband of Ḫuzziya's sister, reorganized the country. He became king by dispossessing his predecessor after he had made an attempt on his life. He tried to stop the violence which had dominated the history of the royal house by regulating the succes-

[5] A. Ungnad, *Die Venustafeln und das neunte Jahr Samsuilunas* (MAOG 13/3, 1940); *AfO* 13 (1940) 145-146; S. Smith, *Alalakh and Chronology* (1940).

[6] Albright, BULLETIN 88 (Dec. 1942) 28-33.

[7] Cornelius, *Klio* 35 (1942) 1-16; van der Waerden, *BSAW*, math.-phys. Klasse 94 (1943); *Ex Oriente Lux* 10 (1945-1948) 414-424.

[8] Boehl, *Bibliotheca Orient.* I/6 (Nov. 1944) 101 f.; *King Ḫammurabi of Babylon in the Setting of his Time* (1946) 7-12; K. Schubert, *WZKM* 51 (1948) 21-33.

[9] The genealogy of Šuppiluliumaš is securely established by the hieroglyphic inscription on the Niṣantaş in Boğazköy; see H. G. Güterbock, *Boğazköy* (*APAW* 1935, phil.-hist. Klasse 1) 63 ff. On the early history of the dynasty see Götze, *MAOG* 4 (1928) 64 f. and A. Goetze, *Kizzuwatna* 75 ff.

[10] E. Forrer, 2BoTU p. 18*, argues that Muršiliš continued to reign for 15-20 years after his return from Babylon.

[11] For the reason see the brief characterization of the text below.

sion to the throne. The codification of the law may be his work.[12] It is known that he concluded a treaty with Išputaḫšuš of Kizzuwatna, an indication of his activity in the field of foreign affairs. His reign must have been a fairly long one.

(7) The next king, Alluwamnaš, was married to Ḫarapšekiš, who probably was a daughter of Telepinuš. An impression of his seal, originally affixed to a land-deed, has come down to us.

(8) Of Ḫantiliš nothing is known beyond his name. Ḫarapšekiš seems to have continued as Tawananna during his reign which probably was of only short duration.

(9) The next royal "couple" are Zidantaš and Iyayaš.

(10) They are followed by Ḫuzziyaš and Šummiriš. A land-deed of Ḫuzziyaš on which his seal is impressed has survived.

The next kings are Tudḫaliyaš and Arnuwandaš whose queens were Nikkal-mati and Ašmu-Nikkal respectively;[13] Tudḫaliyaš is Šuppiluliuma's great-grandfather.

Before giving the evidence on which these statements are based, it is necessary to characterize briefly the two main sources at our disposal for the period concerned. They are the so-called Telepinuš text on the one hand, and the lists of sacrifices for kings and queens on the other.

The part of the Telepinuš text[14] on which we draw here is a chronicle in which the king of that name—see No. (6) above—contrasts the power of the Hittites in the good old days when they were internally united with their weakness in the period of internal strife that preceded his accession to the throne. The facts which are included in his report are selected from this point of view; we cannot expect them to familiarize us with all the important events.

The usefulness of the lists[15] depends on the question as to whether they are arranged chronologically. The question is to be answered in the affirmative; wherever we can control them the order is indeed systematic and chronological.[16] Confirmation comes now from the new frag-

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[12] Cf. A. Goetze, *Kleinasien* 103 with fn. 7.

[13] The name of Ašmu-Nikkal is to be restored in 2BoTU 24 II 21. This follows from the fact that she was the daughter of Nikkal-mati; see Güterbock, *MDOG* 74 68 and *Siegel aus Boğazköy* 1 44. In some texts containing the Telepinuš myth (*KUB* XXXIII 15 7; 21 III 20) the Stormgod is said to have been angry at Ašmu-Nikkal, the reason for which the story and the pacification ritual it motivates has been preserved.

[14] KBo III 1 = 2BoTU 23A with the duplicates 2BoTU 23B = KUB XI 1, 2BoTU 23C = KBo III 67, 2BoTU 23D = KUB XI 5, 2BoTU 23E = KUB XI 6, 2BoTU 23F = KUB XI 2 and 2BoTU 23G = KUB XXXI 2 (belongs really to C) with the additional piece KUB XXXI 17. A partial translation (§§ 1-9, 18-31, 49-50) is contained in Joh. Friedrich, *Aus dem hethitischen Schrifttum* (*Der Alte Orient* 24, 3) 7 ff., 21 f. Sturtevant and Bechtel, *A Hittite Chrestomathy* 175 ff. offer §§1-12, 18-33, 49-50.

[15] *2BoTU* 24, 25, 26, 27, 28, 29 commented upon by E. Forrer on pp. 13*-29*.

[16] Forrer's assumption (2BoTU p. 14* ff.) proves basically correct. The problem lies in the selection of the entries rather than their sequence.

ment Bo 18/g (MDOG 76 46) providing us with a list of the earlier kings and queens from Labarnaš to Ammunaš (No. (4) above).

Here then is the evidence for the statements made above:

(1) Muršiliš: Tel. §§8-9 and BM 96152 (L. W. King, *Chronicles Concerning Early Babylonian Kings* 121 ff. and 15 ff.) rev. 10. The capture of Babylon by the Hittites was always remembered as a high point of their national history, see, e. g., KUB XXIV 3 II 46 ff.

(2) Ḫantiliš: Tel. §§10-18, sections which are unfortunately much mutilated; remnants of an Akkadian version are KBo I 27 (see KUB IV p. 50b) + KUB III 85 + Bo 223/g (*MDOG* 76 43). The Kaškean invasion is alluded to in the following texts: Ḫattušil (A. Götze, *MVAG* 29,3 and 34,2) III 46 ff. (for the passage in question KUB XXXI 13, an addition to text F = KUB XIX 65, is a new witness [17]); KUB XXV 21 III 2 ff. (see below); KUB XXI 29 I 11 ff.[18] For the fortification of Ḫattušaš and the Ḫatti country see 2BoTU 20 (= KBo III 57) III 7 ff. The name of Ḫantili's wife Ḫarapšiliš was confused already by the Hittite historiographers with that of Ḫarapšekiš, the wife of Alluwamnaš.[19]

(3) Zidantaš: Tel. §19. Note 2BoTU 23B (= KUB XI 1) I 32, a line which establishes the kings relationship to the dynasty.

(4) Ammunaš: Tel. §§20-21. An inscription of Ammunaš followed that of Anittaš on KUB XXVI 71 (= 2BoTU 30), see Otten, MDOG 76 45. Ammunaš and his queen Tawannaš are mentioned on Bo 18/g (MDOG 76 46). Cf. furthermore 2BoTU 25 I 15.

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[17] Read now as follows:

46 *nu* URU*Ne-ri-ik-ka$_4$-aš ku-it IŠ-TU U$_4$-UM* I*Ḫa-an-ti-li*
47 *ar-ḫa ḫar-ga-an-za e-eš-ta na-an*
48 EGIR-*pa ú-e*[*-da-aḫ*]-*ḫu-un* KUR.KUR.MEŠ-*ia ku-e I-NA* URU*Ne-ri-ik-ka$_4$*
49 *a-ra-aḫ-za-an-da e-eš-ta* [*nu* UR]U*Ni-e-ra-an* URU*Ḫa-aš-ti-ra-an*
50 ZAG-*an i-ia-nu-un na-at-za ḫu-u-ma-an* [1R-*n*]*a-aḫ-ḫu-un*
51 *n*[*a-at*] *ar-kam-ma-na-li-uš* [*i-ia-nu-un* HUR.SAG]*Ḫa-ḫar-wa-aš-za-kán*
52 [UR]U*Ma-ra-aš-ša-an-da-aš-ša* L[Ú.MEŠ.KÚR?*k*]*u-it IŠ-TU* URU*Ne-ri-ik*
53 *Ù IŠ-TU* URU*Ḫa-ak-piš-ša da-ma-aš-ša-an ḫar-kir nu-za ḫu-u-ma-an-da-an*
54 1R-*aḫ-ḫu-un*

"Nerik which since the days of Ḫantiliš had been in ruins
I rebuilt. Also the lands which are
around Nerik—Neraš and Ḫaštiraš
I made (their) border, subjugated all of them
and made them liable to tribute. The mountain country Ḫaḫarwaš
and (the town of) Maraššandaš which [the enemy]
(operating) out of Nerik and Ḫakpiš had oppressed, all of it
I subjugated."

[18] A treaty of Ḫattušiliš:

11 URU*Ti-li-ú-ra-aš* URU-*aš IŠTU* UD.KAM I*Ḫa-an-ti-li*
12 *dan-na-at-ti-iš e-eš-ta na-an A-BI-IA*
13 I*Mur-ši-li-iš* EGIR-*pa ú-e-te-et a-še-ša-nu-ut-ma-an*
14 *na-a-ú-i* SIG$_5$-*in*

"Tiliuraš, the town, which had been uninhabited since the days of Ḫantiliš, my father Muršiliš had rebuilt it, but he had not yet properly resettled it." Cf., in the same text, II 3 ff.

[19] See Otten, *MDOG* 76 46 f.

(5) Ḫuzziyaš: Tel. §8.

(6) Telepinuš: Tel. §8 ff. The Telepinuš text is his work. Treaty with Išputaḫšuš of Kizzuwatna: KUB XXX 42 (cf. Güterbock, MDOG 73 32 f.); KUB XXXI 81 and 82; KUB IV 76.[20] An impression of the seal of Išputaḫšuš was found at Tarsus (*AJA* 39 535 ff. and 40 210 ff).[21]

(7) Alluwamnaš: KUB XXVI 77. For Ḫarapšekiš [21a] cf. Otten, MDOG 76 46 f.; 2BoTU 24 II 1; 25 I 19; 26 6; 27 7; KUB XXXI 74 II 12. The king's seal is known from an impression excavated at Boğazköy (H. G. Güterbock, MDOG 74 70 ff.; Siegel aus Boğazköy I No. 86 (p. 51 f.)).

(8) Ḫantiliš: [22] 2BoTU 24 II 5; 27 7.

(9) Zidantaš: 2BoTU 24 II 7 ff.

(10) Ḫuzziyaš: 2BoTU 24 II 11 ff.
His seal (H. G. Güterbock, l. c. No. 85) is impressed on the land-deed VAT 7436 (Güterbock, l. c. I 74 f. "Text 2").

To summarize then, event (I) is separated from event (II) by no less than nine reigns of which at least two, namely those of Ḫantiliš and Telepinuš, were fairly long. The nine kings in question represent 5 + x, most likely 7, generations.[23] In terms of years this should mean a period of roughly 200 years. Since event (II) is fixed at ca. 1450 B. C., we would on this basis, place event (I) around 1650 B. C. Thus, of the four proposed solutions for the problem of Babylonian chronology, (c) and (d) prove impossible, (b) is unlikely, leaving (a) the one that is to be preferred.

This result can be further confirmed by a closer examination of the text KUB XXV 21 col. III which presents us with a chronological statement connecting the Kaškean invasion (see above under (2) with Tudḫaliyaš, the son of Ḫattušiliš, who lived around 1250 B. C.

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[20] Cf. Weidner, *AfO* 14 76 fn. 3.

[21] Cf. furthermore A. Goetze, *Kizzuwatna* 73 f.

[21a] Ḫa-ra-ap-ši-k[i?] KUB XXXIII 19 III 12; 31 8, a queen with whom the Stormgod is said to be angry, is in all probability this queen. Cf. above fn. 13 and H. Otten, *Die Überlieferungen des Telipinu-Mythus* 63 f.

[22] Cavaignac (*RHA* 24 237 ff., particularly 239 with fn. 11) has thrown suspicions on our Nos. 8-10, considering them as a duplication of Nos. 2, 3 and 5. Hardy, *AJSL* 58 216, under the influence of the new short chronology, has taken this up. There is no valid reason for such a procedure; the repetition of names in a dynasty is nothing to be astonished about. Since I reviewed Forrer's 2BoTU in *OLZ* 1927 1082f., the following features have modified the situation: (1) The new list Bo 18/g (*MDOG* 76 46) enumerating sacrifices for early kings from Labarnaš to Ammunaš (our No. 4 above) has been made known; (2) The chronological position of the royal couple Arnuwandaš-Ašmu-Nikkal has been changed (Güterbock, *MDOG* 74 68; A. Goetze, *Kizzuwatna* 76 f.); (3) To the seal of Ḫuzziyaš (VAT 7436) those of Alluwamnaš and Išputaḫšuš have been added. These seals begin a series which is continued by that of Arnuwandaš-Ašmu-Nikkal (KBo V 7) (cf. H. G. Güterbock, *Siegel* I 53 ff.). There is no reason to date any specimen of the hieroglyphic script prior to Telepinuš (cf. B. Landsberger, *Sam'al* 109 fn. 258).

[23] Ḫuzziyas (No. 5) and his brother-in-law Telepinuš (No. 6) have been counted as one generation. Ḫantiliš (No. 8) has been skipped in this count because he never appears with a queen of his own.

KUB XXV 21 III 2 ff.

2. *nu* URU*Ne-ri-ik-ka$_4$-an* LÚ.MEŠ [*Ga-aš-ga-aš* [24]]
3. *PA-NI* [1]*Ḫa-an-ti-ILI*[LI] *ḫar-ga*[*-nu-wa-an ḫar-kir* [25]]
4. *nu ka-ru-ú 500* [26] MU.KAM *ka*[*t-ta . . . . . .*]
5. *dan-na-at-te-eš ki-it-t*[*a-an-ta-at*] [27]

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6. [D]U URU*Ne-ri-ik-ka$_4$-ma-kán I-*[*NA* URU*Ḫa-ak-piš?* [28]]
7. *še-er e-eš-ta nu-uš-ši* 1 EZE[N *zé-e-ni* [29]]
8. 1 EZEN *te-ši-ia e-eš-šir* [×]
9. *nam-ma-ma-aš-ši* EZEN *Ú-UL* [*e-eš-ta*]
10. *nu-kán* ŠAG KUR.KUR.ḪÁ URU*Ḫa-at-t*[*i . . . . . .* [30]]
11. BAL.ḪÁ-*uš-ša ki-ša-an-t*[*a-at*]
12. *nu* KUR.KUR.ḪÁ URU*Ḫa-at-ti a*[*r-ḫa ḫar-ak-ta* [31]]

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13. GIM-*an-ma-kán ú-uk* [1]*Tu-u*[*d-ḫa-li-ia-aš*]
14. *ša-ra-a iš-pár-za-aḫ-ḫu-un*[ *nu-mu*]
15. [1]*Ḫa-at-tu-ši-li-iš* LUGAL[.GAL *ša-ra-a da-a-aš*]
16. [*nu-m*]*u A-NA* [D]U URU*Ne-ri-ik*[*-ka$_4$* LÚ SANGA *i-ia-at*]

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17. [GIM-*an-m*]*a A-BU-IA* DINGIR.*LI*[*M-iš ki-ša-at*]
18. [ . . . . . ]

"[The Kaške]ans [had] destroyed Nerik
at the time [32] of Ḫantiliš.
For 500 years [. . . .]
it had already lain uninhabited.

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But the Stormgod of Nerik was up
i[n Ḫakpiš] and they used to celebrate for him
one festival [in the fall]
and one festival in the *teša-*,
there [was] no other festival for him.
And in the Ḫatti countries there happened

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[24] Cf. the texts quoted above and KUB IV 1 (translated in *Ancient Near Eastern Texts Relating to the Old Testament* 354 f.) and KUB XVII 21 (translated ibid. 399 f.).

[25] Cf. Ḫatt. III 47 (above fn. 17); KUB XXI 19 III 13.

[26] I have to thank Mesdames Muazzez Çığ and Hatice Bozkurt for collating this passage for me. On the original the number has been corrected, but 5 ME is the reading which it suggests; that is to say Güterbock's copy is quite correct. The fact that the scribe hesitated is not without significance. What he finally wrote down is the result of some reflection.

[27] Cf. KUB XXI 29 I 5 (*dan-na-at-ti-iš e-eš-ta*).

[28] Cf. KUB XXVIII 80 IV 4: *ma-a-an ku-u-ru-ra-aš* MU.ḪÁ-*aš* (5) EZEN URU*Ne-ri-ik* URU*Ḫa-ak-mi-iš-ši* (6) *iš-šu-wa-an da-i-e-er* "when, during the years of upheaval, they undertook to celebrate the festival of Nerik at Ḫakmiš."

[29] For the pair *zeni-teši* see von Brandenstein, *Orient.* NS 8 79.

[30] A noun parallel with BAL is missing here.

[31] Cf. XIX 9 I 6 (*ar-ḫa ḫar-ak-ta*).

[32] Literally "in front of."

[wars] and revolts
and the Ḫatti countries [were] r[uined].

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But when I Tudḫaliyaš
became of age,[33]
Ḫattušiliš, [the great] king [took me]
[and made] me [priest] of the Stormgod of Nerik.[34]

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[But when] my father [became] god,
[ . . . . . ]

The point which is of particular interest for the present discussion is the information that 500 years passed between Tudḫaliyaš and Ḫantiliš. If we try to appraise the value of this chronological statement, we must stress from the outset the obvious fact that 500 is a round number. However, the well-known historical sense of the Hittites justifies the belief that the figure given is not a merely fictitious invention and must be true within a reasonable margin of error. We must try to find out how the Hittite historiographers arrived at that figure.

The suggestion may be advanced here that they did so by computing the generations which had passed between the two kings. It was pointed out above that from Ḫantiliš (No. 2 above) to Ḫuzziyaš (No. 10) we have to reckon with 7 generations. From his successor Tudḫaliyaš to the Tudḫaliyaš in question, there are—both kings included—7 additional generations.[35] This makes 14 generations in all. If we count 3 generations to the century, as seems reasonable, we arrive at approximately 500 years. It is likely that the Hittites figured in the same way.

The average assumed was certainly too high. If we take the 500 years at face value, we would have to place Ḫantiliš at 1750, a figure which would fit in best with the old " low " chronology. As was readily admitted above, this date is too high and a reduction is necessary. However, since it is impossible to assign, over a protracted period of time, to a generation as little as 18 or 21 years, solutions (d) and (c) above are excluded. Solution (b) which assigns ca. 25 years to a generation is barely possible. Solution (a) with 400 years—i. e. one generation = 28 years—remains preferable.

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[33] See A. Goetze, *Kizzuwatna* 23 f.

[34] Cf. Ḫatt. IV 76 ff.

[35] They are (1) Tudḫaliyaš, (2) Ḫattušiliš, (3) Tudḫaliyaš, (4) Šuppiluliumaš, (5) Muršiliš, (6) Muwatalliš and Ḫattušiliš, (7) Tudḫaliyaš.

# ANCIENT COPPER REFINING

James L. Kelso

The first smelted copper was too impure to be used for either the better grades of hammered ware or castings because it was incompletely fused and contained ashes and earth. The discovery of charcoal, whose heat value is more than twice that of dry wood and whose ash content is only 1.5% was a magnificent forward step, especially when the smelting was done in a furnace with forced draft. First the blow pipe, then the Syrian hand bellows, and finally the efficient Egyptian foot bellows supplied the forced draft. The chemical problem involved in the smelting process was a simple one, namely to reduce the oxide and carbonate ores to an impure copper metal. A good forced draft was all that was needed. The removal of the impurities was left for a second heat treatment. In the earliest periods the ancients did not use sulphide ores as they were too difficult to smelt and refine. Later these ores were roasted before smelting, thus reducing the sulphur content to 7%.

The refining of a smelted copper, especially when it was to be used for tools, presented two distinct chemical problems. The first was to remove trouble-making impurities. This was an easy task since some of the impurities would be volatilized and a good percentage of the others would enter into the slag if the refining fire was sufficiently hot and the forced draft properly applied to the molten metal. All ancient smelted coppers show some impurities remaining, but the amount is seldom injurious. Indeed, in many cases the impurities actually improved the copper for the particular use for which it was intended.

Although plenty of air was needed in oxidizing the impurities, a part of this oxygen united with some of the copper itself forming cuprous oxide ($Cu_2O$). When the oxygen content of the molten metal was sufficiently high to remove the impurities, the $Cu_2O$ content was most likely too high to make a good copper for either hammer work or casting. Thus in getting the original impurities out of the smelted copper a new troublesome "impurity" was introduced. Strictly speaking, the $Cu_2O$ actually made an alloy with the copper and was not an impurity, but nevertheless it was a decided troublemaker for the refiner.

His second major problem then was to get rid of any excess of this $Cu_2O$. It was solved by deoxidizing the copper to the correct percentage of $Cu_2O$ which was necessary for the particular use to which that copper would be put. This is done today by a process called poling and it was doubtless done somewhat similarly by the ancients. They were, of course, unaware of the chemical problems involved but their trial and error methods in metallurgy produced a good copper just the same. The proportion of $Cu_2O$ in ancient copper does not differ greatly from modern standard coppers.

After the copper was molten and its slag skimmed off, a piece of green hardwood was thrust into the molten metal. The green stick caught

fire quickly and created hydrocarbon gases whose reactions broke down a part of the $Cu_2O$, thus reducing the oxygen content of the metal. Meanwhile the molten metal was kept covered with charcoal, which not only served to keep the metal molten but also prevented further oxidation. If too much green wood was used, however, then the resultant copper became over-poled, porous and brittle and of no commercial use.

The metal was tested by taking a small sample and allowing it to cool. If its surface was level or just slightly lifted in tiny wrinkles, then the metal was ready for use. When broken it had a rose-red silken fracture. Its $Cu_2O$ content would run about 0.4% and today this is still the most commonly used malleable copper, i. e. "tough-pitch" copper. If the copper swelled as it cooled, showed blow holes and ruffled surface or broke open, then it lacked sufficient oxygen and needed more exposure to the air blast. Its fracture would be orange or yellowish. If the copper showed a surface with a deep depression and perhaps a cavity, had a sonorus ring and a dark brick-red color, then it had too much oxygen which should be removed by more poling. These rule-of-thumb tests were of invaluable assistance to the ancient refiners and explain why the $Cu_2O$ content in ancient coppers does not normally vary greatly from that in standard coppers of today.

Ancient refined coppers, however, usually differ from modern coppers in that the former show a higher percentage of impurities. This plus the unusual appearance of the consequent eutectoids is a good test for modern fakes as they seldom show these ancient characteristics.

The problem of the interpretation of the chemical impurities and alloys in ancient copper is one for the metallurgist rather than the archaeologist as it is a highly complex task. The evaluation of the percentages of both the original impurities and the $Cu_2O$ is in part dependent upon the presence or absence of other impurities and alloys; in part upon the particular purpose for which the copper is used; in part upon whether the copper is cold worked or hot worked, etc. The metallurgists, however, have discovered that the percentages of impurities and alloys often differ in ancient coppers from those in modern coppers. The latter must be trouble-free for fast machine work, and therefore the percentage variations must be slight. Ancient hand worked coppers, however, could have a much wider range as each piece could be treated individually in accordance with its characteristics. It is to be noted that the percentages of impurities in efficient ancient coppers are often higher than in corresponding work today.

To summarize, the analyses of ancient copper objects show that those craftsmen normally used a copper with a reasonably correct percentage of impurities or alloys, depending upon the particular purpose for which the object was to be used. Wares which do not fit this category were probably either the best materials available at that time and place or the craftsman was deliberately catering to a cheap market. Occasionally we find the chemical analysis of a tool to be exceedingly puzzling. In such a case, however, the tool was probably never intended to be used as a tool, but was to serve as a votive offering. This odd formula would be perfectly satisfactory for such a purpose. In small cheap cast objects,

queer formulas have been found but this is to be expected as much of such ware was made from scrap metal of all kinds. Occasionally even large percentages of gold and silver are found in such objects. The ancient coppersmith was indeed a skilled craftsman with a surprising empirical knowledge of the various copper formulas indispensable to the multiple phases of his work.

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## SUMERIAN WISDOM LITERATURE: A PRELIMINARY SURVEY

Samuel Noah Kramer

The field of Sumerian literature is no "strange pasture" to William Albright. Among his earliest scholarly contributions there are several devoted to one or another of the Sumerian literary works,[1] and even his latest writings reveal a keen interest in this field of research. It is therefore a pleasure and a privilege to present on the occasion of Albright's sixtieth birthday the following preliminary survey of one of the Sumerian literary genres, that commonly known as "wisdom." Brief and summary as the sketch is, it should prove not without value to the interested student and scholar and provide them with some idea of the promise which Sumerian wisdom literature holds for their future researches.

Up until some twenty years ago, practically nothing was known about Sumerian "wisdom." But little relevant material had been copied and published, and even that little remained largely unrecognized and misinterpreted. It is only in recent years, as a result of the publication of five volumes filled with copies of Sumerian literary texts,[2] and with the help of much of the still unpublished part of the Nippur literary collection of the University Museum that a deeper insight into the character and variety of the Sumerian wisdom compositions has become possible.[3] Thus it can now be seen that Sumerian "wisdom" consists of five categories: [4] (1) proverbs; (2) miniature essays; (3) instructions and precepts; (4) essays concerned with the Mesopotamian school and scribe; (5) disputes and debates.

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[1] Cf. e. g. *An Indexed Bibliography of the Writings of William Foxwell Albright* (New Haven, 1941), nos. 11, 13, and 17.

[2] TRS (H. de Genouillac, *Textes religieux sumériens du Louvre*) I and II, SEM (E. Chiera, *Sumerian Epics and Myths*), STVC (Chiera, *Sumerian Texts of Varied Contents*), and SLTN (Kramer, *Sumerian Literary Texts from Nippur*).

[3] For an earlier statement, one that should be modified in light of the present survey, cf. SLTN, pp. 35-40.

[4] In the Sumerian literary catalogue CBS 29-15-155 (cf. Bulletin 88, 10 ff.) the last 13 compositions, to judge from the titles, are all wisdom compositions; so, too, probably nos. 25, 30, 31, and 43. The Louvre catalogue (cf. Bulletin 88, 16 ff.) adds at least one more wisdom text (no. 46). For the compositions listed in the catalogues which can now be restored wholly or in part, cf. notes 5, 16, 21, 24. Two wisdom compositions which do not seem to fall in any of these catagories are TRS 80-83 (cf. Falkenstein, *Zeitschrift für Indogermanische Forschungen* 60, 113-120), and *TRS* 39.

Sumerian proverbs,[5] like proverbs the world over, are brief pithy sayings which depend for their effect on extreme terseness of expression; on the unexpected turn of phrase; on evocation and connotation rather than straight statement of fact. As in the case of the Biblical proverbs, they may consist of the single line,[6] or the couplet. The couplet, moreover, again as in the Biblical proverbs, may be characterized either by antithetical[7] or synonymous[8] parallelism, or it may form a continuous sentence.[9] Finally, the Sumerian proverb may consist of three, four, or more lines and thus approach the form of the miniature essay.[10]

The number of Sumerian proverbs, to judge from the extant material, runs into the hundreds, and it will take the combined efforts of numerous scholars over many a year before they can be fully and definitively translated and interpreted. Moreover, from the point of view of arrangement, the ancient Sumerian proverb collections show at least two types. In the one, the order seems to be quite haphazard and devoid of any particular guiding principle, at least according to our best understanding at the moment. In the second type, the Sumerian scribes arranged their material according to subject matter, though the reason for the particular choice and order of subjects is still obscure. Thus in the extant text of our best preserved example, we find the proverbs arranged in the following order of subjects: fate, the poor, the scribe, the fox, the ass, the ox, and the *kalû*-priest.[11]

The second of the Sumerian wisdom categories, the miniature essay, is now represented by a considerable number of fairly well preserved examples,[12] but unfortunately the texts are so difficult that it will be advisable to await a better understanding of their contents before they

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[5] The major published proverb material is *STVC* 1-9 (*STVC* 1 is no. 46 of the Louvre catalogue mentioned in the preceding note), 14-15 (cf. SLTN, p. 40, comment to nos. 142-153), 123, and SLTN 142-153. Published earlier but in the main unrecognized by the copyists, are *PBS* I 2 No. 117; PBS XII, 29, 51; PBS XIII, 38, 50; (*Hilprecht Anniversary Volume*) 23, and the obverse sides of SLT 69, 189, 190, and 193. In addition there is a considerable number of unpublished tablets and fragments which the writer was in a position to utilize. As for the bilinguals published by Langdon in AJSL 28, 217 ff., and by Meek in RA 17, 121-3, 146, 148, 154, 155, 157, 158, 159, it is now obvious that the Sumerian is the original, and not the Akkadian, as has been the tendency to assume hitherto. At least one of the bilingual proverbs is actually found in the Sumerian collections (Bu. 80-7-19, 130 obv. 3-6 = STVC 3, IV, 28-9), and no doubt a good many more such equations will be uncovered in the future.

[6] Cf. e. g. STVC 4 i 8; STVC 1 obv. ii 29-30.

[7] Cf. e. g. HAV 23 rev. iv 7-10.

[8] Cf. e. g. STVC 1 obv. ii 24-7.

[9] Cf. e. g. STVC 1 obv. i 15-18.

[10] Cf. e. g. STVC 9 ii 3-8.

[11] So on the ten-column tablet CBS 13980, which is still unpublished. Of the published material the following can now be seen to belong to this type of proverb collection: STVC 6, 9, 14, 15; SLTN 142, 151; SLT 69, 190; HAV 23; PBS XII, 51; all these duplicate CBS 13980. Note, too, that SLT 193 contains proberbs concerned with "house," and that STVC 7 contains proverbs concerned with the young woman and the wife.

[12] Cf. SLTN 128-34 (note comment *ibid.*, p. 39), BE XXXI, 28 (= SEM 114), 36, 42 (= TRS 6), also probably VS X, 204 and TRS 39.

can be more definitely characterized and described. They range in size from four and five lines—these may therefore also be considered as proverbs—to more than fifty lines, and a number of them end in what may possibly be ironic or sarcastic phrases, such as: "And you are a man!" or "And you are my rival!" [13]

Of the third category, instructions and precepts, we now have at least two collections.[14] The one is a group of precepts attributed to Shuruppak, the father of Ziusudra, who is purported to have uttered them as instructions to his son.[15] The second document purports to be a collection of instructions given by a farmer to his son and is attributed in the colophon to the Sumerian god Ninurta, who is there described as "the trustworthy farmer of Enlil." [16] The composition consists of 108 lines which can now be almost entirely restored with the help of a one-column tablet found last year by the joint expedition to Nippur of the Oriental Institute and the University Museum. This document which is particularly significant for the history of agriculture is now in the process of being translated by Jacobsen, Landsberger, and myself; and a scientific edition including text, transliteration, and commentary, should be available in the course of this year.

The fourth type of Sumerian wisdom composition is that devoted to the scribal school and the scholar, a rather popular theme with the Sumerian schoolmen, and for obvious reasons. At least three of these can now be restored wholly or in large part. One, containing a description of a schoolboy's day in school, has already been published under the title "Schooldays: A Sumerian Composition Relating to the Education of a Scribe" (JAOS 69, 3 ff.). The text of the remaining two is now in the process of being reconstructed and should be available in the near future.[17]

The fifth and last type of wisdom composition is that of the dispute or debate between two rivals, each of whom may personify a season, animal, plant, occupation, implement, metal, or stone.[18] The dispute genre seems to have been a high favorite with the Mesopotamian men of letters.

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[13] *ù za-e lú-lu$^{lu}$-me-en* (BE XXXI, 28, 33); *ù za-e gaba-ri-mu-*[*me-en*] (SLTN 132 obv. 7).

[14] Note that SLTN 135, a poorly preserved tablet with uncertain contents, may be a collection of instructions if the *na-ri-$^{d}$nin-urta* of the last line is to be taken seriously.

[15] Cf. for the present JCS 1, 33, note 208.

[16] Cf. for the present SLTN, p. 30, comment to nos. 59-60; to the list of texts there given should be added the Ur tablet copied by Gadd and discussed by Landsberger, MSL 1, 150 ff. This composition is no. 53 of the Nippur catalogue mentioned in note 4.

[17] Cf. for the present SLTN, p. 36, comment to no. 114 and to nos. 115, 117 (cf. also now JAOS 69, 11 ff.); a large fragment dug up last year by the Joint Nippur Expedition will help considerably in the restoration of this composition. For fragments of several other *é-dub-ba* compositions, cf. SLTN, p. 38 (comment to nos. 113, 116); PBS I, 2, nos. 96, 98.

[18] This category was formerly termed "fable." But as Landsberger has well pointed out (JNES 8, 295 f.), the fable frequently utilizes the "debate" or "dispute" as a literary device to obtain its effect, but is not identical with it. Cf. also Jacobsen, *The Intellectual Adventure of Ancient Man*, p. 165 ff.

We already have the text of seven of them [19] wholly or in part; these are: "Summer and Winter;" [20] "Cattle and Grain"; [21] "Shepherd and Farmer"; [22] "Pickaxe and Plow"; [23] "Tree and Reed"; [24] "Bird and Fish"; [25] "Silver and Bronze." [26] Stylistically they tend to show the following features: First comes a mythological introduction concerned with the creation of the two protagonists; [27] then follows the argument between the two rivals which goes back and forth several times and in which each of the rivals "talks up" his own value and importance and "talks down" those of his opponent. The dispute is finally resolved by a deity [28] whose decision is absolute, and is accepted gracefully by the loser.[29]

## ARCHAEOLOGY AND A POINT IN HEBREW SYNTAX

THEOPHILE J. MEEK

Archaeology has been put to many strange uses in its time, but never before, so far as I know, has anyone used it to elucidate a point in Hebrew syntax, and yet that is what I propose to do in this paper.

An expression that appears rather often in the Old Testament is *l*$^{e}$*fî ḥereb*, used ordinarily with the verb *hikkāh*. All the standard versions (the King James, the English Revised, the American Revised, and the Jewish Revised) everywhere translate the phrase "to smite with the

[19] It is not impossible that SLTN 131 (a tablet containing several miniature essays, cf. note 6), obv. i contains a brief debate between the millstone and the *gul-gul*-stone.

[20] Cf. SM (Kramer, *Sumerian Mythology*) 49 ff., where it is treated as a myth under the title "Emesh and Enten: Enlil chooses the Farmer God"; cf. also BULLETIN 105, 7 ff. For the change of title, cf. Landsberger's excellent detailed study in JNES 8, 248-297.

[21] Cf. SM 53-4; BULLETIN 105, 7. This composition is no. 17 of the Nippur catalogue and no. 11 of the Louvre catalogue; cf. note 4.

[22] Cf. SM 101-103, *Intellectual Adventure of Ancient Man*, 166-68; JCS 2, 59-69; JNES 8, 295, note 151.

[23] Cf. STVC 117, 119, 128; BE XXXI, 50; SRT 26; the University Museum has a considerable number of unpublished pieces belonging to this composition.

[24] Cf. TRS 53; PBS 42. This composition is perhaps no. 30 of the Nippur catalogue and no. 54 of the Louvre catalogue; cf. note 4.

[25] Cf. VS X 204 obv. ii; TRS 31.

[26] Cf. SRT 4; *Intellectual Adventure of Ancient Man*, 165; JNES 8, 275 ff.

[27] This is true of "Summer and Winter," "Cattle and Grain," and "Tree and Reed"; "Shepherd and Farmer" shows quite a different type of mythological introduction (cf. literature cited in note 22), while in case of the remaining three "disputes," the introduction is missing. It is because of the significant mythological contents of at least some of these "disputes" that they may in a sense be characterized as myths.

[28] Enlil makes the decision in favor of summer in "Summer and Winter"; for "Shepherd and Farmer," cf. the literature cited in note 22; in case of the remaining five "disputes," the end is missing.

[29] So in case of "Summer and Winter" and probably "Shepherd and Farmer," the only compositions in which the closing details are extant. [Further study shows that STVC 1 (cf. note 5) is not a collection of proverbs, but a lengthy essay in which the author, writing in the first person, recites his sufferings and woes.]

edge of the sword," but this is of course absolutely wrong because פִּי here does not mean "edge," despite G. R. Driver's citation of it to confirm this meaning of the cognate in the Middle Assyrian laws,[1] and *l-* cannot possibly express means or instrument in Hebrew. When we pass from the Old Testament translators we find that the commentators, if

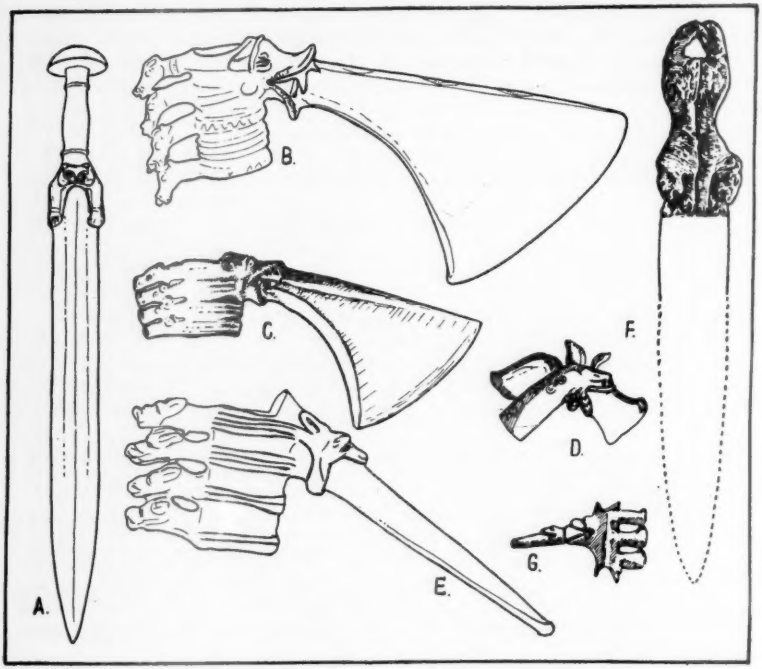


FIG. 1

they mention the matter at all, regularly interpret the *l-* as the *l* of norm, "according to the mouth of the sword," i. e., "as the sword can devour," meaning, so they say, "without quarter."[2] That is of course possible, but it seems to me that there is a better interpretation, which rather strangely never seems to have occurred to anyone.

Why not take *l-* here in its most natural and usual sense, the terminative, "to smite into the mouth of the sword"? With this interpretation the sword is presented under the metaphor of a devouring monster,

[1] See G. R. Driver and John C. Miles, *The Assyrian Laws* (1935), p. 460, bottom.
[2] See, e.g., S. R. Driver, *International Critical Commentary on Deutcronomy* (1895), p. 154.

and in confirmation of that we have the evidence of statements like those in Deut. 32:42; II Sam. 2:26; 11:25, where the sword is definitely spoken of as devouring its prey, and rather oddly we have also the evidence of archaeology to support our interpretation.

Right across northern Syria, northern Mesopotamia, and north-western Iran, in the excavations all the way from Ras Shamra to Nihavand, swords and battle-axes have been found in which the blade is represented as the tongue sticking out of the open, ravenous mouth of a lion or dragon, which constitutes the base of the sword hilt or the head of the axe, as the case may be. A number of these are illustrated in the swords and axes from various excavations appearing in Fig. 1.[3] The animal motif on the sword apparently came in course of time to be conventionalized into a crescent because this constitutes the base of many swords from the Near East,[4] and the transition from the animal head to the crescent is very apparent in Fig 1, A.

So far as I know there is no way of reproducing the metaphor of the Hebrew in a translation that makes acceptable English, but at least the thought of the original is retained in the translation "to put to the sword," which I have used in *The Bible: An American Translation.* Here is another of the many instances where the Hebrew contains more than the English translation can possibly reproduce and the metaphor is completely lost in the process of translation, even though the general thought of the original is retained. When the expression appears with verbs like *ḥlš* "to weaken," *hmm* "to discomfot, to drive into a panic," and the Hiphil of *ḥrm* "to devote," its accurate translation into English is quite impossible, and the best that we can do is to approximate the meaning of the original and lose the metaphor entirely.

Sometimes the hilt of the sword or the head of the axe consists of one animal head as in Fig. 1, A to E and G, but sometimes it consists of two, as in Fig. 1, F, and the famous axe from Ras Shamra, with the blade issuing from the mouths of two lions.[5] This raises the question whether the expression "two-edged sword" should ever appear in the translation of the Old Testament, because "two-edged" is not the literal meaning of the expression. In Judges 3:16, for example, the words read literally "a sword which had two mouths," and that reminds us of the swords and axes that we have just mentioned. It would be most unlikely that a sword would be called two-edged in the Old Testament because ancient Near Eastern swords were regularly two-edged, but only some of them were two-mouthed. Just how we are to translate the Hebrew expression into English is difficult to say. "Two-edged" is clearly not correct, and "two-mouthed" or "double-mouthed" would be meaningless to the ordinary reader.

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[3] Reproduced from Claude F.-A. Schaeffer, *Ugaritica* (1939), p. 118, Fig. 107; for other examples see, e.g., Schaeffer, *op. cit.*, Figs. 101-3, 110 f., and Pl. XXII; René Dussaud, *Syria*, XI (1930), pp. 250 f., Figs. 8-12, and Pl. XLII *bis.*, 2; *British Museum Quarterly*, VI (1932), Pl. XXIX, *b*; *Illustrated London News*, No. 5251 (Dec. 9, 1939), p. 867, Fig. 2; André Parrot, *Tello* (1948), p. 109, Pl. 26, *b*.

[4] See, e.g., W. H. Flinders Petrie, *Tools and Weapons* (1917), Pl. XXXIII, Nos. 1, 4, 7, 11-13, 23, 35; Pl. XXXIV, Nos. 42-47; Pl. XXXV, No. 95.

[5] Illustrated by Schaeffer, *op. cit.*, Pl. XXII.

# WHERE DID EZEKIEL RECEIVE THE CALL TO PROPHESY? *

HARRY M. ORLINSKY

If the cycle of Ezekiel studies in the past quarter century, one which differed sharply from the past by its extremely critical attitude towards the reliable character of the Book as an historical document, is beginning to come to a close,[1] there will be two important reasons for this newer outlook: (1) Archaeology has made it necessary to give up the attitude of extreme scepticism towards the essential reliability of much of the biblical writings for the reconstruction of the biblical period in history. For this quite radical change in outlook no single scholar is more responsible than Albright. (2) It seems to me that the first decisive argument against the tendency to look upon most of the book of Ezekiel as pseudepigraphic was the brilliant article by Albright in 1932 on "The Seal of Eliakim and the Latest Pre-Exilic History of Judah, with some Observations on Ezekiel" (*JBL*, 51, 77-106). More recently, his understanding of the book of Ezekiel may be discerned in the doctoral dissertation written by his student, C. G. Howie (see n. 1).

The preserved text of Ezekiel states explicitly that the prophet received his initial "call" in Babylonia: "Now it came to pass . . . I was among the captives by the Chebar canal, that the heavens were opened, and I saw visions of God . . . And I looked, and behold a stormy wind came out of the north, a great cloud, with a fire flashing up, so that a brightness was round about it . . . This was the appearance of the likeness of the glory of the Lord . . . And He said to me, 'Son of man, I send you to the children of Israel . . .'" (1: 1, 4, 28; 2: 3).[2] Many scholars however felt that this statement was not true, and that Ezekiel

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* Part of a paper ("Where did Ezekiel Begin, and Continue to Prophesy?") read by title at the 86th meeting of the Society of Biblical Literature, New York, Dec. 27, 1950.

[1] Cf. S. R. Driver in the last (1913) revision of his excellent *Introduction to the Literature of the Old Testament*, p. 279: "No critical question arises in connexion with the authorship of the book, the whole from beginning to end bearing unmistakably the stamp of a single mind." Cf. also such surveys as S. Spiegel, "Ezekiel or Pseudo-Ezekiel?" (*Harvard Theological Review*, 24 [1931], 245 ff.); G. A. Cooke, *ICC on Ezekiel* (1936); R. H. Pfeiffer, *Introduction to the Old Testament* (New York, 1941), 525 ff.; W. A. Irwin, *The Problem of Ezekiel* (Chicago, 1943), 3-30; C. G. Howie, *The Date and Composition of Ezekiel* (1950. Vol. IV in the Monograph Series of the *JBL*), passim.

It should be recognized that a, if not the, paramount factor in this previously stable and consistent attitude towards Ezekiel's Book was the assumption that Wellhausen's conception of the Documentary Theory was essentially correct, and the Theory dated the P(riestly) Document in relation to the book of Ezekiel whose traditional date in the first half of the sixth century was then not seriously questioned.

[2] Most scholars are agreed that there are two separate chronological materials in 1:1-2, and many interpretations of the "thirtieth year" in v. 1 have been offered (cf. the survey in Howie, Chap. II). However, our argument does not depend at all on the manner in which the problem of the "thirtieth year" is resolved.

received his call in Judah and prophesied in that land; and the question whether he ever did prophesy in Babylonia became secondary in importance.[3]

Over six hundred had elapsed since the Israelites had established themselves in a land of their own, and almost four hundred years had gone by since Solomon had carried out David's intention to erect a dwelling place on Zion for the Lord.[4] While all the peoples and all the extremities of the world were ultimately subject to the Lord's will, Israel alone was His covenanted people, and the land of His dwelling place was uniquely holy. In all this period, no person outside of the Holy Land had received a " call " from the Lord, had experienced the spirit of God coming down upon him and into him, had been commanded in a vision to become His prophet.[5]

Ezekiel is the first person to become a prophet outside of God's holy territory. His call is thus utterly unique, and quite out of line with precedent and tradition. One could readily understand how a prophet who received his initial call in Babylonia would deny this fact, and claim instead to have received divine authority initially in Judah, on holy soil. It is inconceivable however that a prophet who received his call in Jerusalem, in Judah, would suppress this fact, and claim instead a foreign land as the birthplace of his prophetic career. So far as I am aware, no one who has rejected the biblical statement has attempted to answer the question, What could Ezekiel (or a redactor) have hoped to gain by shifting the locale of the initial call from Judah (if so it was) to Babylonia?

Reading the book of Ezekiel with no preconceived ideas, the initial call in Babylonia becomes perfectly clear and normal. Ezekiel did receive the same kind of authority to prophesy as his predecessors who were in the Holy Land. And in common with them, he received it directly from the Lord. There was only one difference between Ezekiel's call and theirs, one which derived from Ezekiel's presence in a foreign land, namely, the Lord came from Judah to Babylonia, and with His holy presence on foreign soil commissioned Ezekiel as His prophet. In fine,

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[3] Cf. the survey in Howie, Chap. I (to which may be added Spiegel, JBL, 56[1937], 403-8; " When did Ezekiel Go into Exile? [in Hebrew; pp. 2-8 in the *Nissan Turoff Volume*, New York, 5798-1938]).

[4] One cannot overestimate the importance of this concept (now known strikingly also from the Ugaritic texts) in Israelite history and in the prophetic tradition. It was the primary concern of all the prophets before 587 that the Temple remain standing and functioning, and after 587 that the Temple be rebuilt (cf. Ezekiel, Second Isaiah, Haggai, Zechariah). Ezkiel's great concern with the rebuilding of the Temple derives directly from the fact that he is the first prophet in Israel to be active when God's Temple was no more.

[5] The rabbis were acutely aware of this fact; cf. the survey by Spiegel (§3 " Ezekiel or Pseudo-Ezekiel? "). As a matter of fact (*Mekilta*, ed. J. Z. Lauterbach, Vol. I, Tractate Pisha I, pp. 6-7; with which cf. the Targum at 1:3), " . . . Some say, He [viz., God] had already spoken with him in the [Holy] land, and then He spoke with him outside of the land . . . You can learn from the following that the Shekinah does not reveal itself outside of the land . . ." But surely no one would use this rabbinic statement (or Tractate Shirata in the *Mekilta*, Chap. VII [pp. 54-5 in ed. Lauterbach]) as the basis for the argument that Ezekiel received his prophetic call in the Holy Land.

Ezekiel's call as described in Ezek. 1 is essentially the same as Isaiah's call as described in Isa. 6, except that whereas Isaiah came to the Lord, it was the Lord who, of necessity, came to Ezekiel.[6]

## SYRIANS AS PICTURED IN THE PAINTINGS OF THE THEBAN TOMBS

JAMES B. PRITCHARD

The paintings upon the walls of the New Kingdom tombs at Thebes constitute the principal source for our knowledge of the appearance of the inhabitants of Palestine, Syria, and the upper Euphrates during the second half of the second millennium B. C. Well over two hundred individual figures on these paintings from twenty-one of the numbered tombs [1] are sufficiently preserved to serve as evidence for the appearance of the foreign peoples to the northeast of Egypt. The dates of the tombs, covering roughly a century and a half, can usually be fixed approximately within the reign of a king, or in a few cases to the reign of two kings.[2]

The importance of these paintings for a knowledge of the appearance of Asiatics was emphasized by W. Max Müller as early as 1893.[3] Later Müller undertook to copy and to study in more detail some of the more important paintings.[4] A more thorough attempt to record photographically the evidence of the Theban tombs was made by the so-called Eduard Meyer Fremdvölker-Expedition, under the direction of Max Burchardt and Friedrich Koch during the winter of 1912-13.[5] Photo-

[6] While it does not affect our argument, I do not agree with those (e. g., Driver, *op. cit.*, p. 280; J. Morgenstern, *Hebrew Union College Annual* 16 [1941], 65-7) who would see in the term *šīphôn* in Ezek. 1:4 anything more than the conventional direction of one coming from Judah to Babylonia, even as one who comes to Judah from the region of Mesopotamia may likewise be said to come from the "north"; cf. the Malbim (which my friend Prof. Sidney B. Hoenig called to my attention), *loc. cit.*; D. H. Müller's very useful *Ezechiel-Studien* (Berlin, 1895), p. 28; H. G. May, JBL, 56 (1937), 317 and nn. 25-26.

[1] Tombs 17, 39, 40, 42, 55, 58, 63, 78, 84, 85, 86, 89, 90, 91, 100, 119, 120, 131, 162, 226, 239.

[2] The dates given by Alan H. Gardiner and A. E. P. Weigall, *A Topographical Catalogue of the Private Tombs at Thebes* (London, 1913), p. 44, are generally followed. Exceptions are dates for tombs which have been more carefully studied. To Thut-mose III are assigned 39, 84 (*JEA*, XXVII, 96), 86, 119 (perhaps to Hatshepsut—Thut-mose III), 131; to Amen-hotep II, 17 (?); to Thut-mose III—Amen-hotep II, 42, 85, 100; to Thut-mose IV, 63; to Amen-hotep III, 58, 89 (?, see *JEA*, XXVI, 131), 120 (see *Bulletin of Metropolitan Museum of Art* [*BMMA*], ii, Nov. 1929, 35), 226; to Thut-mose IV—Amen-hotep III, 90 (or to Amen-hotep II—Thut-mose IV, as Norman de G. Davies, *Tomb of Puyemrê*, [abbrev.: *Puyemrê*], I, 108), 91, 239 (?); to Thut-mose III—Amen-hotep III, 78; to Amen-hotep IV, 55; to Amen-hotep IV—Tut-ankh-Amon, 40; to the 18th dynasty, 162. Davies, *Puyemrê*, I, 108, assigns to the reign of Thut-mose III the following: 42, 78, 84, 85, 86, 100.

[3] *Asien und Europa nach altägyptischen Denkmälern*, Leipzig.

[4] *Egyptological Researches* (abbrev.: *Researches*), Washington, I, 1906, II, 1910.

[5] E. Meyer, *SPAW* (phil.-hist. Cl.), 1913, pp. 769-801. The number of the negatives

graphs were taken of seventeen tombs containing representations of foreign peoples.[6] The rich material contained in the 756 negatives taken by the expedition has never been fully published. The more important photographs were used by W. Wreszinski in his *Atlas zur altägyptischen Kulturgeschichte*.[7] Many of the paintings within the Theban tombs have been carefully copied by Norman de Garis Davies and Nina de Garis Davies and reproduced in outline or in color in the *Theban Tomb Series*,[8] the publications of the Metropolitan Museum of Art,[9] and the *Journal of Egyptian Archaeology*.[10] Important, too, are the lavishly published volumes, *Ancient Egyptian Paintings* (abbrev.: *Paintings*), by Nina de Garis Davies and Alan H. Gardiner. In addition to the published materials a photographic record of many of the tombs was made by Mr. Harry Burton for the Metropolitan Museum of Art. This important collection of photographs has been available for the present paper.[11]

Most of the scenes in which Asiatics are portrayed on the walls of the tombs deal with the subject of tribute brought by foreigners to Egypt. Usually the picture is composed of several registers of subject peoples bringing the products of their lands. Labels are frequently attached to the rows of figures, or to the foreign chiefs who appear at the heads of their respective delegations. The products carried are varied, sometimes labeled, but not always appropriate to the region from which the men are said to have come.[12] Foreign captives are also represented.

In the table given below a tabulation has been made of the types of dress in which these men are shown. The men are bearded and either are labeled as Syrians [13] or, if not, obviously were intended to represent people from the northeast of Egypt. As many of the paintings have

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listed in this catalogue will be referred to hereafter as *Fremdvölker*. The Metropolitan Museum of Art in New York has a complete file of the prints of these negatives.

[6] All tombs mentioned in footnote 1, except 58, 120, 131, 162.

[7] Wreszinski reproduces photographs from all the tombs mentioned in footnote 1, except 40, 55, 58, 120, 131, 162. The pictures in the *Atlas* will be referred to as *Wreszinski*.

[8] Nina de Garis Davies and Alan H. Gardiner, *The Tomb of Ḥuy* (abbrev.: *TTS*, IV), London, 1926; Nina de G. Davies and Norman de Garis Davies, *Tomb of Menkheperrasonb*, etc. (abbrev.: *TTS*, V), London, 1933; Norman de G. Davies, *The Tombs of Two Officials of Tuthmosis the Fourth* (abbrev.: *TTS*, III), London, 1923.

[9] Norman de G. Davies, *The Tomb of Puyemrê at Thebes*, New York, 1922-23; Norman de G. Davies, *The Tomb of Rekh-mi-Rê' at Thebes*, (abbrev.: *Rekh-mi-Rê'*), 2 vols., New York, 1943; Norman de G. Davies, *Paintings from the Tomb of Rekh-mi-Rê'* (abbrev.: *Paintings Rekh-mi-Rê'*), New York, 1935. *BMMA*, ii, Dec. 1924; March 1926; Dec. 1928; Nov. 1929; Dec. 1930.

[10] Tomb 84, *JEA*, XXVII, 96-8; tomb 85, *JEA*, XX, 189-92; tomb 89; *JEA*, XXVI, 131-36; tomb 162, *JEA*, XXXIII, 40-6.

[11] Tombs which have been photographed by Burton are 39, 42, 55, 63, 78, 84, 85, 86, 100, 131. Photographs in the files of the Metropolitan Museum will be referred to by the letter *T* followed by the number of the negative.

[12] For detailed discussion of objects see P. Montet, *Les reliques de l'art syrien dans l'Égypte du Nouvel Empire*, Paris, 1937, and the critical review by Norman de G. Davies, *JEA*, XXIV, 253-4.

[13] Labels include such terms as "northern countries," "Further Asia," "Retenu (Upper and Lower)," "chief of Naharain," "chief of Kheta," "chief of Tunip," "chief of Kadesh," "chief of Lebanon," etc.

been poorly preserved, it is difficult to determine exactly the number of the examples of the various types. The numbers tabulated here serve only to give some idea of the distribution of the various types of dress over the periods from which the paintings come. For the convenience of those who may wish to look up the pictures, references have been given in the footnotes to the publications and collections of photographs. The letters A, B, C, D refer to the drawings given on page 39.[14]

| Thut-mose III—Amen-hotep II [15] | | | | | Thut-mose IV—Amen-hotep III [16] | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Tomb | A | B | C | D | Tomb | A | B | C | D |
| 131 | | 4 | | | 78 | 1 | | 1 | |
| 39 | 2 | 3 | | | 63 | 5 | | 1 | 16 |
| 100 | 7 | 23 | | | 58 | | | 4 | |
| 119 | 4 | 1 | | | 89 | 7 | | | 15 |
| 84 | 4 | 11 | | | 120 | | | 2 | |
| 86 | 4 | 5 | | | 226 | | 1 | 2 | |
| 42 | (8) | 9 | | | 90 | 15 | | | 2 |
| 85 | | 6 | | 9 | 91 | | | | 9 |
| 17 | 5 | 1 | | | 239 | | | 6 | |
| | | | | | 55 | | | | |
| Amen-hotep IV—Tut-ankh-Amon [17] | | | | | Eighteenth Dynasty [18] | | | | |
| 40 | 8 | | 15 | | 162 | 3 | | 20 | |

In the tombs which can be dated approximately to the period of Thut-mose III—Amen-hotep II (1490-1421) two types of dress are rather

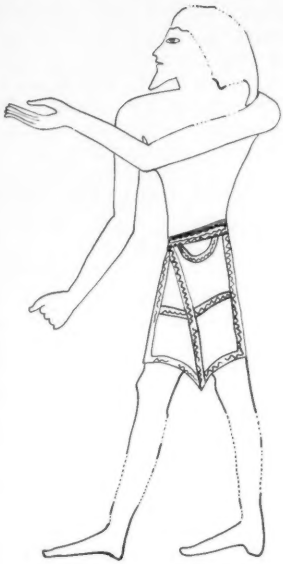
[14] A is from *Paintings*, XXIV; B, *ibid.*, XXI; C, Lepsius, *Denkmäler*, III, 116a; D, *Paintings*, XLII.

[15] Tomb 131: *BMMA*, ii, Mar. 1926, 41 ff. Tomb 39: (1) Wreszinski, I, 150; *Puyemrê*, I, pl. XXXI. (2) Wreszinski, I, 149; *Fremdvölker*, 779; T 644; *Puyemrê*, I, pls. I, XXXVI, XXXIIIA. Tomb 100: Wreszinski, I, 335, 336, 337; T 2562, T 2034, T 2023, T 2030, T 2028; *Paintings Rekh-mi-Rē'*, pls. X, XI, XII, XXII; *Fremdvölker*, 775-8; *Rekh-mi-Rē'*, II, pls. XXI-XXIII. Tomb 119: Wreszinski, I, 340; Fremdvölker. 592-3. Tomb 84: Wreszinski, I, 269; *Fremdvölker*, 738-9; T 1870; *JEA*, XXVII, pl. XIII, pp. 96-8; *Researches*, II, pls. 23-8. Tomb 86: *Wreszinski*, I, 273-7; *Fremdvölker*, 596-600, 721-2; *TTS*, V, pls. IV, V; *Paintings*, pls. XXI, XXIV; *Researches*, II, pls. 1-22; T 1781, T 1778, T 1779. Tomb 42: (1) Wreszinski, I, 88; *Fremdvölker*, 801A-804; T 3435; *TTS*, V ch. III, pls. 33-5. (2) Wreszinski, I, 168; *Fremdvölker*, 799-801; T 3434; *TTS*, V, pl. 36. Tomb 85: Wreszinski, I, 4; *Fremdvölker*, 594-5; T 2574; *JEA*, XX, pl. XXV. Tomb 17: Wreszinski, I, 115; *Fremdvölker*, 730-3.

[16] Tomb 78: Wreszinski, I, 247-8; *Fremdvölker*, 743-7; T 1964-6; *Paintings*, comment on pl. XXXVIII. Tomb 63: (1) Wreszinski, I, 56a[1]; *Paintings*, XLII. (2) *Fremdvölker* 750-1; T 2765-6. Tomb 58: *Paintings*, LX. Tomb 89; Wreszinski, I, 285; Fremdvölker, 763; *JEA*, XXVI, pls. XXIII-XXIV. Tomb 120: *BMMA*, ii, Nov. 1929, pp. 35 ff. Tomb 226: Wreszinski, I, 372; *Fremdvölker*, 734-7; *TTS*, V, pls. XLIII, XLI; *Paintings*, LVIII. Tomb 90: Wreszinski, I, 288; *Fremdvölker*, 762; *TTS*, III, pls. XXVIII, XXIX. Tomb 91: Wreszinski, I, 290; *Fremdvölker*, 758-60. Tomb 239: Wreszinski, , 373; *Fremdvölker*, 621-2, 726-7. Tomb 55: *Fremdvölker*, 805; T 1528; *BMMA*, ii, Dec. 1924, p. 49, fig. 5.

[17] Tomb 40: *Fremdvölker*, 589, 618; Lepsius, *Denkmäler*, III, 115-6a; *TTS*, IV, pl. XIX.

[18] Tomb 162: *JEA*, XXXIII, 40-6. pl. VIII.



A

B

C

D

consistently employed for portraying Syrians. The first (A) is a simple, kiltlike garment which extends from the waist to the knees, or slightly above the knees. In the several tombs of this period there are some twenty-six (not counting the eight indistinct figures in 42) well-preserved examples of this type of dress. The garment seems to be a simple piece of cloth wrapped about the waist, usually decorated by red and blue lines at the borders. In some examples the skirt sags into a point between the legs in front (39, 84), but this point should not be confused with the codpiece which is seen on representations of the dress of the Keftiu. Nor is the skirt worn by Syrians easily confused with the white, wrap-around skirt worn by Egyptians. This form of dress appears as early as the First Dynasty on representations of Asiatics,[19] and again on three bearded figures of the Beni Hasan caravan of the Middle Kingdom, but seems to disappear gradually (remaining, but in a more simplified form, in 78, 63, 89, 90, 40, and 162). It is usually shown on figures interspersed with those wearing the long, sleeved dress (B). Out of the twenty-six examples tabulated, all but one figure is shown wearing long hair, reaching to the ears or shoulders and held in place by a band or fillet around the top of the head. The one exception is a figure on the wall of tomb 119, where the head is shaved.

The other type (B) of dress shown in this period is a long, white, long-sleeved garment extending almost to the ankles. This, too, is usually gaily decorated with blue and red along the edges and with a broad line running down the front of the garment. Some sixty-three examples of this costume are well preserved in the paintings. In the greater number (some forty-five) the man is shown with the hair on his head closely cropped or shaved. Only occasionally does the wearer of B have long hair held in place with a fillet. In three examples the hair is shown red and worn in a kind of shock encircled by a band.

On one painting (85) of this period there appears a third type of dress, a modification of the long-sleeved gown by the addition of a sari-like band wrapped about the lower part of the B garment. This type of dress (D) becomes very popular in the later period and will be discussed below.

Ten tombs may be assigned to the period of Thut-mose IV—Amen-hotep III (1421-1377). The most noticeable change in the representation of Syrians on the walls of these later tombs is the almost complete disappearance of the dominant representation of the earlier period, namely the simple, long-sleeved dress B. While the greatest number of figures wore type B in the early period, there is but a single example of it in the tombs from the time of Thut-mose IV—Amen-hotep III. It seems to have given way to the more elaborate habit shown in figure D, which was basically the custom of the earlier period; but around the skirt was wrapped several times a long, narrow piece of cloth decorated along the selvege. In some cases, as in its earliest appearance on tomb 85, the folds are as many as six or seven and reach from the waist almost to the ankles. The decoration along the border is blue and red in varying combinations. In the first appearance of this type of dress it is worn

[19] Gressmann, *Altorientalische Bilder* (abbrev.: *AOB*), 29.

by the three chiefs in the front of each row and may have represented a higher rank. But there is little evidence for this distinction in the period from which the most examples come.

In the same period, which begins about the last quarter of the 15th century, there appears an entirely new type of dress (C). It seems to consist of a long undergarment reaching in folds almost to the ankles, over which is placed a cape worn loosely over the shoulders. Sometimes the undergarment is represented with long sleeves, but more frequently it is not. Type C continues on figures of Syrians well after the 18th dynasty. It is seen on the figures of bound prisoners before Horemheb in the relief of the Leiden Museum,[20] on the Karnak relief of Seti I depicting the " princes of Lebanon " felling cedars,[21] on the reliefs from the time of Ramses II,[22] and on the glazed tiles from Medinet Habu from the time of Ramses III.[23] The greatest number of examples are found on the famous wall painting of Huy (40) from the time of Amen--hotep IV—Tut-ankh-Amon, where there are some fifteen examples of this type of costume. Interspersed with these figures are eight men wearing only the simple kilt, apparently a dress of servants or porters, to judge from the heavy objects which they carry as tribute. On a staff from the grave of Tut-ankh-Amon there is carved a similar figure of a bound Syrian.[24] Thus, this particular type of dress seems to appear first about the time of Thut-mose IV and continues down into the 20th dynasty, a period of over two and a half centuries.

How trustworthy is this evidence for a knowledge of the dress of people to the northeast of Egypt? [25] The Egyptian artist painted to please his " patron "; he was bound to traditional types, and he did make mistakes (see top row of 86, where a Syrian is labeled Keftiu, and note the boot painted on a Syrian in 84). Nor is it likely that the Egyptian artists were interested in differentiating the various peoples encountered by the armies on their campaigns northward during the New Kingdom. On the other hand Egyptian artists from the time of Thut-mose III onward has frequent opportunity to observe the foreigners who came, or were brought into Egypt. It would be strange indeed if their representations of these people did not catch something of their actual appearance.

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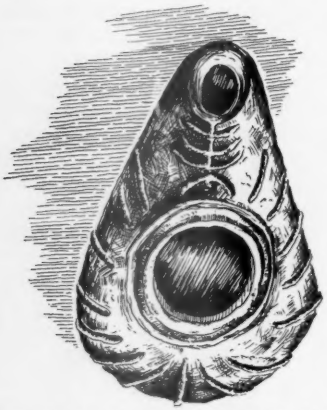
[20] *AOB*, 87.
[21] *AOB*, 93.
[22] *AOB*, 102, 105.
[23] *AOB*, 13.
[24] *AOB*, 18.
[25] See Norman de G. Davies, *BMMA*, ii, Dec. 1930, pp. 29 ff.

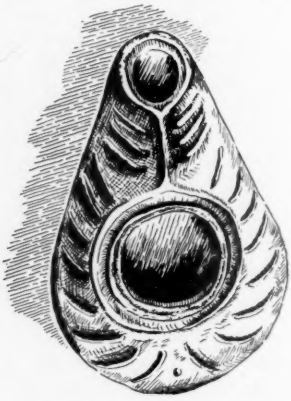
# THE CANDLESTICK DECORATION ON BYZANTINE LAMPS

O. R. Sellers

In Jewish worship the seven-branched candlestick or menorah was a part of the paraphernalia from ancient times. Lamps with seven spouts go back to the time of Solomon. When the menorah with seven pedestals for small lamps, or the true candlestick with sockets for seven candles, was introduced is a matter of conjecture. An early representation is on Maccabean coins of the 1st century B. C.[1] From then on it increases in frequency of appearance. It is carved into the walls of houses, syna-



1. Lamp showing the menorah with tripod. Length, 9.7 cm.; width, 6.2 cm.; height, 3 cm.



2. Lamp with true candlestick pattern disappearing. Length, 9.4 cm.; width, 6.3 cm.; height, 3.2 cm.

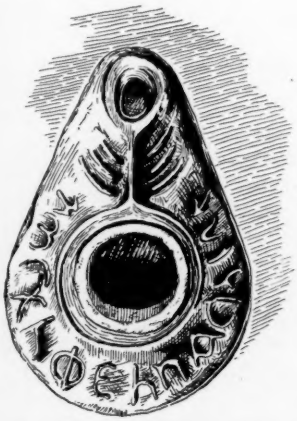
gogues, and burial chambers, on sarcophagi, on amulets; it is set in mosaics; it is burned into glass medallions. In the famous Arch of Constantine the seven-branched candlestick, carried by eight men, seems to have been the most prized trophy taken from the Jerusalem temple when its treasures were carried to Rome.[2]

In the diaspora there was fond memory of the candlestick; in fact, it seems to have become the distinctive emblem in affirming the Jewish faith of the owner of the object on which it appeared. In the Roman period it was a popular design on lamps. Most of these lamps had a closed discus, so that the oil was poured in through the spout; some had

[1] See A. Reifenberg, *Ancient Hebrew Arts* (New York, Shocken Books, 1950). This excellent book pictures outstanding examples of Israelite and Jewish art from the times of the kings to the Roman period. An illustration (p. 69) shows a candlestick on a coin of Antigonus Mattathias (40-37).

[2] A splendid illustration of this scene is shown by Reifenberg (pp. 78-79).

3. Lamp with cross and inscription, *Phos Ch(risto)u pheni pasin kale.* Note the inversion of the lambda. Length, 10 cm.; width, 6.4 cm.; height, 3.4 cm.



4. Lamp witch degenerate "candlestick." The inscription, so far as it can be read, runs from right to left. Length, 9.8 cm.; width, 6.3 cm.; height, 3.5 cm.

5. Lamp with nine branches in the "candlestick." Only a few letters of the inscription, running from right to left, are legible. Length, 9.8 cm.; width, 6.2 cm.; height, 3 cm.

small holes in the discus.[3] Where pagan lamps of this type were apt to have erotic scenes or portraits on the discus the Jewish lamps would have the menorah.

While the base of the candlestick pictured on the Arch of Titus is conical, the candlesticks of the diaspora generally show the base as a tripod, and this is particularly true of those pictured on lamps. This tripod base is shown distinctly on lamps of the Roman period and it was carried into the Byzantine period when the candlestick decoration became conventionalized. In the Byzantine period the closed discus gave way to a large hole in the center of the lamp, so that oil could be poured in easily. The narrow neck of the Roman lamp was widened, giving the Byzantine lamp the shape of a somewhat flattened pear. The candlestick decoration was moved from the discus to the space between the feeder hole and the spout. The tripod then appeared before the large hole; the small hole, through which came the wick, served as the top light of the menorah (Fig. 1).

During the ASOR-McCormick Theological Seminary excavation which D. C. Baramki and the writer conducted in the cave belonging to the family Abu Halimeh of the village of Silat edh-Dhahr, June, 1949,[4] we found more than ninety lamps of this general pattern with some interesting variations on the candlestick. While we found a number of Roman lamps, all the lamps with candlesticks were Byzantine. In the Roman period the candlestick almost invariably has the tops of the branches on one level, as in the Arch of Titus and in menorah representations in synagogues and churches today; but a few Roman candlesticks show the lamps in an arc.[5] The Byzantine lamps from Silat edh-Dhahr all show the arc pattern.

The presence of these candlestick lamps in the Silat edh-Dhahr catacomb raises the question as to whether some of the burials were Jewish. No other distinctly Jewish objects were found in the cave, though there was an abundance of Christian objects—bronze crosses, lamps with cross decorations, and a seal ring showing a Christian knight. It is possible, but not probable, that the burials were mixed Jewish and Christian. It is possible also (as Professor Erwin R. Goodenough has kindly suggested) that the genuine menorah lamps, having magical "power" in them, were purchased by Christians from Jews. In that case it would seem that the Christians took over the menorah as a decoration without thought of what it was. The tripod is lost, there is merely a line joining the circle around the feeder to the circle around the spout, and the six other branches are detached from the stem (Fig. 2).

Christians then produced a lamp of the same shape, but with quite different decorations. Instead of the menorah there was a cross and around the lamp a Greek inscription, *Phos Christou pheni pasin* ("the light of Christ shines on all"), generally with another word, *kale* or

[3] *Ibid.* pp. 144-145.
[4] *The Biblical Archaeologist*, XII, No. 3 (Sept., 1949), pp. 57-63.
[5] Reifenberg, *op. cit.*, pp. 134, 146.

*kala* (Fig. 3).[6] Other lamps, turned out by less learned Christian potters, show a degenerate imitation of the pious inscription on lamps with the degenerate menorah decoration (Fig. 4). As the inscription became further confused the "menorah" also lost its distinctive feature of seven branches. It came to have nine or more branches and eventually became a palm branch (Fig. 5).

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## THE DATE FORMULAE OF SHU-ILISHU OF ISIN

FRANCIS RUE STEELE

As written records of the past are recovered and deciphered they reveal an increasingly clearer picture of ancient times. Some documents contain information which enables a year-by-year reconstruction of the passage of time centuries ago and provides an outline of relative chronology upon which significant events and developments can be placed in proper relationship to one another. For Babylonian history such a chronological framework can be reconstructed in large part from the business records bearing date formulae which served to identify business transactions with specific periods of time. The formulae not only occur singly on individual tablets, but often—when transactions from two or more consecutive years are listed on one tablet—several formulae appear together. In addition, the scribes frequently compiled special lists of date formulae in consecutive order.

Present information provides a fairly complete chain of date formulae for Babylonian history from the early part of the III Dynasty of Ur down to the closing decades of the First Dynasty of Babylon—a period of nearly 500 years—with but one considerable gap. For the time of the Isin Dynasty (ca. 1960-1730) about two thirds of the date formulae are missing entirely, and quite a number more are fragmentary. However, the majority of those preserved, since they contain the name of the reigning king, can at least be assigned to specific reigns. But of these only four which refer to a monarch's accession year can be placed precisely within the regnal periods to which they belong.[1]

Up to the present time only three date formulae for the reign of Shu-ilishu have been published. One of these is the standard formula for the first year of a king's reign and reads *mu* $^{d}$*šu-i-li-šu-lugal-àm*, "The year Shu-ilishu became king."[2] The total length of this king's reign is

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[6] A thorough discussion of lamps with this inscription is by Edgar R. Smothers, S. J., in *Traditio*, Vol. V (1947), pp. 46-49. The *kale* or *kala* remains somewhat of an enigma. The lambda regularly is upside down. The explanation that *kala* (beautiful) advertises the high quality of the lamp raises the question as to whether such a commercial word would have been added to a pious expression; but no better hypothesis has been offered.

[1] See the list of formulae for the Isin Dynasty in the *Reallexikon der Assyriologie* (RLA) II 147 ff., especially nos. 158 and 182 and p. 256 nos. 34! and 85!

[2] *Ibid.* 256 no. 34!

known from other documents to have been ten years.[3] Now, however, we can supply the remaining date formulae—some in part only—and arrange all ten in their proper order. This restoration is based largely upon a tablet discovered in the course of last season's excavations at Nippur.

One of the two places dug on the now famous Tablet Hill at Nippur by the joint expedition of the Oriental Institute of the University of Chicago and the University Museum of the University of Pennsylvania was in a deep cut left by the excavators of half a century ago. A small test pit sunk in this depression during our first season in 1948 showed that the lowest level reached in Hilprecht's time dated to the first half of the Old Babylonian period. Last season's work at this spot took us through the remainder of the Old Babylonian period into a settlement which was occupied during the closing days of the III Dynasty of Ur. Among the objects recovered were about sixty tablets from the time of the Isin-Larsa dynasties, some of which bore dates from the reigns of the first three kings of Isin. One of these tablets is a four-column composite business document of nearly 100 lines in fairly good shape.[4] It bears successive entries recording payments of various commodities made by two men over a period of several years. The economic information is itself of no great importance. But the date formulae at the end of each paragraph, since they include the name Shu-ilishu the second king of the Isin Dynasty, are most welcome.

It is clear from the shape of the tablet as well as the context that the better preserved side is the reverse. Here five complete date formulae are preserved, each containing the royal name. The opposite or obverse side of the tablet is much worn and defaced, consequently the surface is often illegible. Nevertheless, there are traces, in varying degrees, of three earlier formulae, in two of which the royal name is at least partly discernible. One of these is in the middle of column two and the other and earlier at the bottom of column one. Moreover, there is evidence for yet another and still earlier date formula on column one.

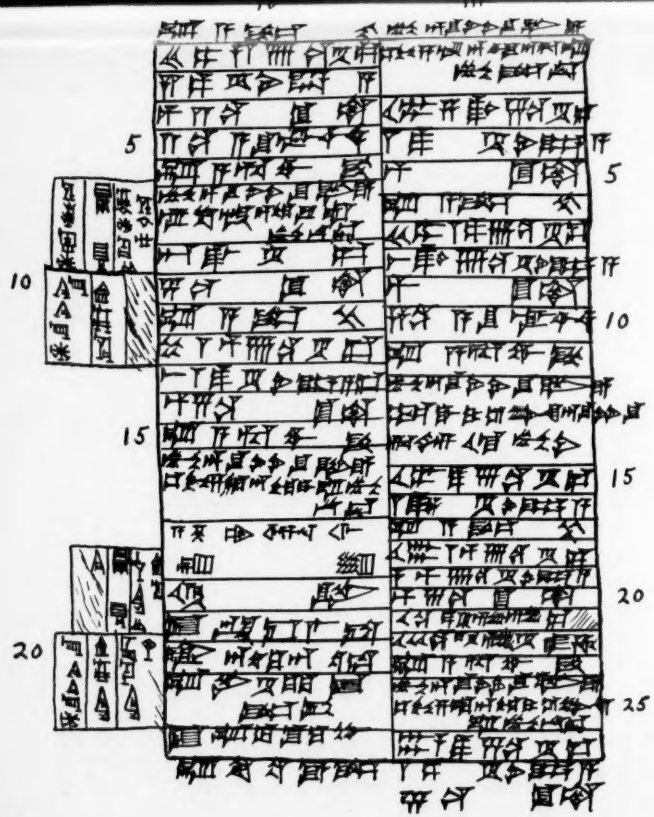
The text of the tablet follows a fairly regular pattern: a series of entries followed by the name Abi-ṭāb plus a second series followed by the name Aḫu-waqar and then a year name. This sequence is repeated clearly six times.[5] Above the date formula at the bottom of column one the name Abi-ṭāb occurs twice.[6] It is therefore clear that somewhere between lines eight and eleven in the broken portion there was another and earlier date name. Moreover line eleven clearly shows traces of the verb form *mu-na-dím*. These traces must represent fragments of the earliest date formula on the tablet. Since it precedes a formula containing the royal name Shu-ilishu, but does not conform to the known

[3] Cf. Jacobsen, *The Sumerian King List*, 1939 (*Oriental Institute Assyriological Studies no. 11*) p. 125.

[4] 2N-T668; 120 x 6.8 cm., 24 + 22 + 28 + 23 11s.

[5] The last paragraph of the reverse alone contains the entries and name of but one man, Aḫu-waqar.

[6] Lines 4 and 18.



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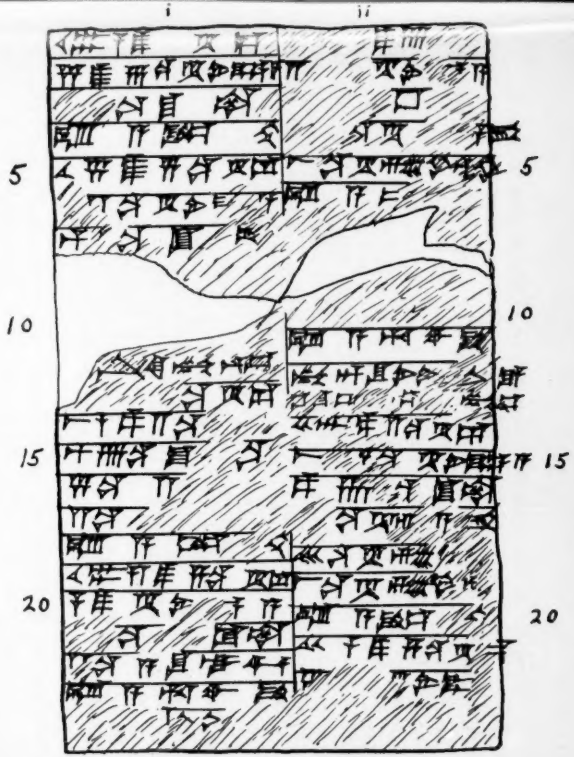

Obv.

formula for his accession year, it must refer to a later but still early year in his reign.

The eight formulae from this tablet together with the previously known formula for the first year give nine of ten formulae for the reign of the second king of Isin. But with one of the ten missing we cannot be sure of the exact place for the eight which follow the first year. However, the last formula on the new tablet recovered this past season at Nippur is paralleled on a tablet discovered at Ur.[7] And another tablet from Ur bears this formula in a slightly different form: *mu-ús-sa* $^{d}$*šu-ì-lí-šu lugal-e* $^{giš}$*gu-za-gal* (?) $^{d}$*nin-gal mu-na-dím*, " the year after Shu-ilishu the king made the dais-throne for Ningal." [8] Now such a year name referring to an earlier event and paralleling an earlier formula can serve one of two purposes. It might be used only as a supplement to the standard formula for the year.[9] Or, on the other hand, it might itself be the official formula for the current year as is frequently the case when, apparently, no outstanding event had occurred since the last year name had been adopted which was deemed of sufficient importance to be used in a date formula.[10]

In the present instance our data are too scanty to allow for a definite decision. Nonetheless, it seems more than likely that the *ús-sa* formula stands for a separate year on the following grounds. In the first place, all nine known formulae belong to years preceding the *ús-sa* formula. Secondly, the tablet bearing the *ús-sa* formula is dated to the tenth month of the year.[11] A tablet written so late would be expected to employ the official date formula for the year. Therefore, it is almost certain that the *ús-sa* formula itself designated the tenth and last year of Shu-ilishu.

Below are the ten formulae as they are now known:

1. *mu* $^{d}$*šu-ì-lí-šu lugal-àm* (RLA. II. 256 no. 34!)
   " the year Shu-ilishu became king."
2. *mu* $^{d}$*šu-ì-lí-šu lugal-e . . . .* KI (?) *mu-na-dím* (I. 11 f.)
   " the year Shu-ilishu the king fashioned the . . . ."
3. *mu* $^{d}$*šu-ì-lí-šu lugal-e* [12] . . . (I. 24)
   " the year Shu-ilishu the king . . . ."
4. *mu* $^{d}$*šu-ì-lí-šu lugal-e . . . . mu-dím.* (II. 12 f.)
   " the year Shu-ilishu the king fashioned the . . . ."
5. *mu* $^{d}$*šu-ì-lí-šu lugal-e* $^{GIŠ}$*gu-za-maḫ an ù* $^{d}$*inanna mu-ne-dím* (III. 1 f.)
   " the year Shu-ilishu the king fashioned the exalted throne for An and Inanna."

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[7] Gadd and Legrain, *Royal Inscriptions* (*Ur Excavations, Texts*) (RIU) no. 213. The omission of *bára* after GIŠ*gu-za* is strange.

[8] Ibid. no. 214. The suggested GAL sign should be read BÁRA.

[9] Cf. e. g. RLA II 144 no. 78b et al.

[10] Cf. e. g. *ibid.* 145 no. 81.

[11] I received this information from Mr. Donald Wiseman of the British Museum where the tablet is stored.

[12] The traces on the tablet appear to parallel the formula *mu* $^{d}$*šu-ì-lí-šu lugal-e* $^{d}$ *šu-nir* $^{d}$*en . . .* (cf. RLA II 256.36).

6. *mu* $^{d}$*šu-ì-lí-šu lugal-e bàd-gal i-si-in*$^{KI}$ $^{d}$*šu-ì-lí-šu ri-im-iš*$_4$*-tar mu-dù* (III. 12 f.)
"the year Shu-ilishu the king built the great gate of Isin 'Shu-ilishu beloved of Ishtar.'"
7. *mu* $^{d}$*šu-ì-lí-šu lugal-e* $^{GIŠ}$*gu-za-bára* $^{d}$*nin-i-si-in-*$^{KI}$*ra mu-na-dím* (III. 24 f.)
"the year Shu-ilishu the king fashioned the dais-throne for Ninisinna."
8. *mu* $^{d}$*šu-ì-lí-šu lugal-e ma-gur*$_8$*-maḫ* $^{d}$*nin-urta-ra mu-na-dím* (IV. 7 f.)
"the year Shu-ilishu the king fashioned the *makurru*-boat for Ninurta."
9. *mu* $^{d}$*šu-ì-lí-šu lugal-e* $^{GIŠ}$*gu-za-bára* $^{d}$*nin-gal-ra mu-na-dím* (IV. 16 f.)
"the year Shu-ilishu the king fashioned the dais-throne for Ningal."
10. *mu ús-sa* $^{d}$*šu-ì-lí-šu lugal-e* $^{GIŠ}$*gu-za-bára* $^{d}$*nin-gal-ra mu-na-dím* (RIU no. 214)
"the year after Shu-ilishu the king fashoned the dais-throne for Ningal."

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## AN EPIGRAPHICAL EXPEDITION TO NORTH-EASTERN TRANSJORDAN

F. V. Winnett

It has long been known that southern Syria is rich in epigraphical remains. Some 6,500 Safaitic, in addition to numerous Arabic, Greek, and Latin inscriptions have been found there.[1] A recent expedition of the American School of Oriental Research in Jerusalem in co-operation with the Jordan Department of Antiquities indicates that north-eastern Transjordan may be equally rich in inscriptional material. On September 16, 1950, a party consisting of Mr. G. W. L. Harding, Chief Curator of Antiquities for Jordan, Dr. Lucetta Mowry, Dr. William H. Morton, Mr. D. C. Baramki, and the writer, together with two Arab assistants, penetrated to Jāwā, thirteen kilometers north of pumping-station H5 on the IPC pipeline. Modern Jāwā consists of a small Bedawin encampment around a weak spring, but the presence of a large fortified site (now in ruins) and a dam across the wady two kilometers to the west shows that it lies on what was once an important caravan route. On the rocks around Jāwā such a large number of Kufic and Safaitic inscriptions were found that it was impossible to record them all in the short time available.

On October 16 a return visit was made to Jāwā, which became a base of operations for an examination of the surrounding territory during the next five days. The personnel of the party was the same as before,

[1] For these, see E. Littmann, *Safaitic Inscriptions* (Princeton University Archaeological Expeditions to Syria, Division IV, Section C, Leyden, 1943), and the *Corpus Inscriptionum Semiticarum*, Part V, vol. 1 (in press).

except that Mr. J. A. Thompson, an Honorary Fellow of the School, replaced Miss Mowry. Through the courtesy of Glubb Pasha an Arab Legion truck and two legionnaires were placed at the disposal of the expedition. Without them it would have been virtually impossible to traverse the difficult terrain. The photographing of the inscriptions was done by Mr. Harding and Dr. Morton; Mr. Baramki copied the Kulfic, and Mr. Thompson and the writer copied the Safaitic. The largest finds of inscriptions were made at Jāwā, Jathūm, and Tell el-'Abd, the latter two sites being close to the Syrian border. Visits were also paid to Deir el-Kahf and Deir el-Qinn but no inscriptions were found there, except a badly weathered Greek inscription at the latter place.[2] At Deir el-Kahf the American School linked hands, as it were, with the Princeton Expedition to Syria which visited this site in 1904-5.[3]

The collation of the copies and photographs of the inscriptional material collected by the recent expedition is not yet complete, but a provisional estimate would be: 700 Safaitic, 100 Kufic, and 7 Greek texts. Many more Safaitic inscriptions were observed, especially on the way back from Tell el-'Abd, but there was no time to copy them. It seems very probable that there are thousands of Safaitic graffiti in northeastern Transjordan awaiting the recorder. It is true that the average Safaitic inscription, taken by itself, is a most unimpressive relic but, taken in bulk, they yield a respectable fund of information regarding the religion, culture, and dialect of the pre-Islamic Arabs of this area, and an effort should be made to salvage them.

A number of the newly found inscriptions are dated by current events. Only one of these will be discussed here. It reads as follows: *lmty bn ḫzn wmrd 'l rm snt 'ty ḥmḏy bṯry ḥlt slm*—" By Mātī b. ḪZN. He rebelled against Rome the year the Persians came to Boṣrā. So, O Allāt, (grant) protection." It is probably the same war which is referred to in DM 554. Dussaud read this inscription as follows: *lmḥwr bn ghtfn bn 'dnt wṣyr bfngt hsnt ḥrb ḥgḏy 'l rm bbṣr ṣqtrz*—" By Muḥawwar b. Ghaṭafān b. 'Odhainat. He came to Fingat (?) the year when ha-Gadhy made war against Rome at Boṣra . . ." Littmann [4] suggested reading the latter part of the inscription as *wṣyr bflgth* (or *ḥn'gth*) *snt ḥrb ḥmḏy 'l rm bbṣr sq wrz*—" and he came with his bands (or his lambs) the year of the war of the Medes (i. e. Persians) with the people of Rome at Boṣrā. Waraz (i. e. Shahwaraz, the general of Chosroes II, 590-628 A. D.) was repulsed." Our inscription, as well as DM's own copy, shows that the emendation *ḥmḏy* is justified.[5] The other emendations are questionable. Our inscription shows that in Safaitic " Boṣrā " was spelled with a final *y*, as it is in Classical Arabic. Therefore, DM's *ṣ* is a mistake

[2] These two places were first described by Dussaud and Macler in their *Mission dans les Régions Désertiques de la Syrie Moyenne* (Paris, 1903), pp. 431, 433.

[3] See H. C. Butler's description in *Publications of the Princeton University Archaeological Expeditions to Syria in 1904-5 and 1909*, Division II, Section A: *Southern Syria*, pp. 145-8.

[4] " Die Erwähnung eines Perserkrieges in den Ṣafā-Inschriften " (*Zeitschrift für Assyriologie*, XVII, 1903, pp. 379-83).

[5] In the Minaean inscription, Hal. 535, the Persians are referred to as *ḥmḏy*.

for *y*. (The top of a Safaitic *ṣ* has the form of *y*.) It follows that *q* should be read as *w*. (The two letters are very similar in Safaitic.) For *bfngth* I would suggest reading *bfngrth* (*bi-fanājirathu*) " with his skilful horsemen." The inscription would then read: " By Muḥawwar b. Ghaṭafān b. 'Udhainat. He came with his skilful horsemen the year that the Persians fought with the Romans at Boṣrā, and he distinguished himself." Muḥawwar is careful not to say on which side he fought, but it is evident from Mātī's inscription, quoted above, that the " Safaitic " Arabs aided the Persian invaders. After the Persians were driven out, Mātī felt it necessary to invoke the protection of the goddess Allāt against the wrath of the Byzantines.

Littmann hesitated to identify the Persian-Roman struggle at Boṣrā referred to in DM 554 with the Persian capture of Boṣrā in 614 A. D. for several reasons: (i) The wars between the Romans or Byzantines and Persians were so frequent that it is difficult to tell which war is meant here. (ii) The absence of Christian influence in the Safaitic texts suggests that all these texts date from a period before Christianity had become widespread in Syria. (iii) The bulk of the Safaitic inscriptions seem to date from the second and third centuries A. D. A seventh century date for one text is, therefore, improbable.

There are a number of considerations, however, which lead one to believe that this distinguished scholar has been, in this instance, overly cautious. In the first place, the only known Persian occupation of Boṣrā took place in 614 A. D. Secondly, it is by no means certain that Christian influence is absent from the Safaitic texts. In an article entitled " References to Jesus in Pre-Islamic Inscriptions," published some years ago,[6] I attempted to demonstrate that the god *YT'*, who appears quite frequently in Safaitic, is none other than Jesus. Thirdly, the argument from the second-third century A. D. date of most of the Safaitic inscriptions is scarcely valid since we are by no means sure when the Safaitic script fell into disuse. Dussard's discovery near en-Nemārah in southern Syria of an inscription, dated 328 A. D., which seemed to be a link between Nabataean and Islamic Arabic, led scholars to assume that the old Arabic scripts were by this time on the way out and that a new script, evolved from Nabataean, was coming into use in North Arabia. But if this were so, how are we to account for the small number of inscriptions in this proto-Arabic script? Up to the present only four have been found![7] The contrast between the number of inscriptions in the Safaitic script and in the proto-Arabic script is so striking that one is impelled to ask, Had the northern Arabs forgotten how to write? And if so, why? What can account for this sudden decline in literacy?

Why should the northern Arabs have abandoned their script in the fourth century A. D. whereas the Arabs of the Yemen continued to use their script right down to the triumph of Islam? Littmann himself has

[6] *The Moslem World*, XXXI (1941).

[7] In addition to the en-Nemārah inscription, we have the Zabad, Harran, and Umm al-Jimal texts, the latter three dating from the 6th century A. D. Some inscriptions found on the Nabataean temple at Ramm may possibly be pre-Islamic (cf. *Rev. Bib.*, xliv, 1935, p. 270).

drawn attention to the fact that the presence of the article *'al* in the en-Nemārah inscription shows that its author did not belong to any of the tribes of the Ṣafā, who regularly employed the article *ha*. Therefore, the existence of this inscription is no proof that the Safaitic script had been abandoned by 328 A. D. There would seem to be every likelihood that the Safaitic and Thamudic scripts continued in use right down to the rise of Islam.

To sum up, the usual assumption that the proto-Arabic script had displaced the Safaitic and Thamudic scripts before the rise of Islam is open to serious question. The paucity of inscriptions in the proto-Arabic script is evidence that it had very little currency before the rise of Islam. How and why it came to be adopted by the founders of Islam is a subject which needs further investigation. One thing is clear: the rise of Islam was an historical phenomenon of sufficient strength to bring about the disuse of the old scripts associated with the days of paganism, but it is doubtful if it is possible to discover any equally powerful cause in the history of Arabia in the fourth and fifth centuries able to effect such a radical change. The arguments advanced above are, admittedly, not conclusive but they are of sufficient strength to warn us that it may yet be necessary for us to revise our ideas about the history of the art of writing in Arabia.

## AN IMPORTANT CORRELATION BETWEEN THE PALESTINIAN AND SYRIAN CHALCOLITHIC

G. Ernest Wright

In a prehistoric seminar conducted by Professor Henri Frankfort at the Oriental Institute (1939-1941) a definite correlation was discovered between one phase of the Chalcolithic cultures of Palestine and those of the 'Amuq valley in Syria. With the generous permission of Professor Robert J. Braidwood I am able to mention it here.

The earliest occupation discovered in the 'Amuq by the Oriental Institute expedition, chiefly at Tell Judeideh, is in Phases A and B (approximately contemporary with the lowest level, V, at Ras Shamra). Above these was Phase C, in which Halafian painted wares were first imported from Northern Mesopotamia and locally imitated. At Judeideh itself this was followed by Phase E, filled with typical Obeid painted ware, whereas Phase F contained pottery of the Early Bronze I type in Palestine.[1]

On the neighboring Tell Kurdu, in an excavated area of some twenty square meters with *ca.* four and one-half meters of depth, there was found

[1] In the preliminary report, *American Journal of Archaeology*, Vol. XLI (1937), pp. 10-11, the cultural sequence at Judeideh was given in levels, numbered with Roman numerals. Level XIV there is now broken down into Phases A-C, and Level XIII is the same as Phase F.

material which is labelled "Phase D," because it appeared just above Phase C pottery and represented an intermediate stage between the Halaf and Obeid periods of Phases C and E on Judeideh. No pottery in Phases A-C nor in E could be directly compared with anything found in Palestine, though the flints of A-B had affinities with those in the Neolithic levels at Jericho (Tahunian II).

Phase D, on the other hand, contained several items which are astonishingly similar to certain ceramic features of Level VIII at Jericho. The following are among the more important:

1. The bow-rim, a peculiar neck and rim which is concave inside and convex outside, in section presenting a bow shape: cf. I Ben-Dor in the Liverpool *Annals of Archaeology and Anthropology*, Vol. XXIII (1936), Pl. XXXII, Type 21. This is a peculiar and yet characteristic feature of both sites.

2. Smeared red wash or slip decoration.

3. Footed bowl or stand with hole in the foot: cf. *ibid.*, Pls. XXXII, Type 32 and XXXIII, No. 2.

4. "Hole-mouth" jar rims of at least three different types: *ibid.*, Pl. XXXII, Types 24 A and B, 25 A.

5. Flat bases of jars.

6. Raised combing or ribbing on the exterior of certain vessels: *ibid.*, Pl. XXXIII, No. 11.

7. A peculiar lug-handle with a triangular section, which seems to belong in the range of the Palestinian Chalcolithic, though not clearly witnessed or published from Jericho VIII itself: cf. G. M. Shipton, *Notes on the Megiddo Pottery of Strata VI-XX*, p. 45 and Chart of Stratum XX forms, No. 5; Robert Koeppel, S. J., *Teleilat Ghassul* II (1940), Pls. 77 : 8 and 78 : 9-12.

These features would appear to mean that Jericho VIII, with related material at other sites, is to be considered as roughly contemporary with 'Amuq, Phase D, which in turn is transiitonal between the Halaf and Obeid painted pottery cultures of Mesopotamia.

In my dissertation on *The Pottery of Palestine from the Earliest Times to the End of the Early Bronze Age* (1937), a main concern was to indicate the Chalcolithic and pre-Bronze Age date of the Ghassulian culture. This is now definitely established, but the cultural sequence of the Palestinian Chalcolithic is still far from clear, owing to the fact that no continuous stratification through the whole period has yet been found. The Ghassulian, Jericho VIII, and gray-burnished ware [2] cultures all belong to this age. That the last mentioned immediately precedes Early Bronze I, and is, therefore, to be dated in the second half of the fourth

[2] Called in *PPEB* the "Esdraelon Culture" because at that time it had been found only in northern Palestine. Since the war it has been discovered at Tell el-Far'ah, northeast of Shechem: R. de Vaux, O. P. and A. M. Steve, O. P., *Revue Biblique*, LIV (1947), pp. 400-403; Tome LV (1948), pp. 546-548; Tome LVI (1949), p. 114.

millennium, is clear from almost every stratified site in which it has been found. In nearly all the early mounds in the northern part of the country thus far excavated (notably Tell el-Far'ah, Megiddo, Beth-shân, Khirbet Kerak and 'Affûleh) the sequence is fairly continuous from the "Esdraelon Culture" through Early Bronze II (evidently approximately contemporary with at least the first two Egyptian dynasties). Following what seemed to be the indications at Jericho and Beth-shân, I listed the Jericho VIII culture, in 1937, as post-Ghassulian and pre-Esdraelon —therefore as "Middle Chalcolithic." In favor of this arrangement may be listed the following points:

1. The introduction of red-washed and red-slipped wares seems closely connected with the "Esdraelon" and Early Bronze Age techniques, since they are not typical of the Ghassulian.

2. Rudimentary ledge handles suggest the first stage of the typical ledge handle forms of the following periods.

3. Certain items in the lowest Beth-shân level, XVIII, seem to continue into Level XVII of the "Esdraelon" period (e. g. flat loop handles and "hole-mouth" rims); and the same seems to be true in the two lowest strata at Tell el-Far'ah.

4. The fact that at both Beth-shân and at Tell el-Far'ah what seems to be a culture of the Jericho VIII type is followed directly by the "Esdraelon" gray-burnished ware.

Recently Professor Albright has suggested that the Jericho VIII culture should be placed before the Ghassulian.[3] He would include in this phase Jericho VIII itself, Father de Vaux's "Énéolithique Moyen" at Tell el-Far'ah, most of Megiddo XX, and several sites in the Wadi Ghazzeh in southern Palestine, but not Beth-shân XVIII. The last mentioned he would leave as post-Ghassulian and transitional from Ghassul to the "Esdraelon" culture. In favor of this revision may be listed the following points:

1. The flint culture of Jericho VIII gives definite evidence of being pre-Ghassulian, as observed both by Mrs. Joan Crowfoot Payne and Mrs. Robert J. Braidwood (see Albright, AJA, *loc. cit.* for further details).

2. At 'Affûleh in the Esdraelon Sukenik found a few pieces of Ghassulian material directly below the grey-burnished ware culture (*Archaeological Investigations at 'Affuleh*, Jerusalem, 1948, Pl. I).

3. At Ghassul itself only the topmost level, Stratum IV, has been excavated, and such resemblances as exist between it and the Jericho VIII culture may actually indicate that the latter represents an earlier stage of the Ghassulian. On the other hand, it is quite clear that in all strata at the bottom of the large mounds, and particularly in Megiddo

[3] *American Journal of Archaeology*, Vol. LIII (1949), pp. 213-214; *The Archaeology of Palestine* (Pelican Books, 1949), pp. 65 ff.; BULLETIN, No. 114 (April, 1949), p. 18.

XX, considerable mixture of material is present with the result that no clear picture can be obtained.

The synchronism between Jericho VIII and Phase D of the 'Amuq is perhaps a further argument in favor of the pre-Ghassulian dating of the former. This means that the Ghassulian in Palestine is roughly contemporary with Phase E and the Obeid painted pottery culture, whereas Phase F would be roughly coeval with the Uruq and Proto-literate periods of Mesopotamia and in Palestine with the " Esdraelon " culture and at least the first phase of Early Bronze I.

The problem of Beth-shân XVIII is certainly not completely solved by this dating, nor is the situation at Tell el-Far'ah, where grey-burnished ware follows directly Jericho VIII material—except to assume that the first does not belong in the Jericho horizon and that a gap exists at the latter site which is to be filled with the Ghassulian.

If Jericho VIII is post-Ghassulian, then Ghassul must be correlated in time with Phase C and the Halaf period in the 'Amuq; Jericho VIII with Phase D; the " Esdraelon " culture with Phase E (Obeid in the 'Amuq); and Early Bronze I with Phase F (assuming that neither in Syria nor in Palestine does there exist a phase like that of Uruq in Mesopotamia). On the whole, I should prefer this second arrangement, except that it spreads out the Chalcolithic materials of Palestine unduly and, more important, it seems to violate the clear evidence for the pre-Ghassulian dating of the Jericho VIII flint culture.

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